

Psychological Review

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PUBLISHED BIMONTHLY BY THE
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Manuscripts should be submitted to the Editor-elect, Richard L. Solomon, Emerson Hall, Harvard University, Cambridge 38, Massachusetts.

PUBLISHED BIMONTHLY BY THE
AMERICAN PSYCHOLOGICAL ASSOCIATION, INC.
PRINCE AND LEMON STREETS, LANCASTER, PA.
1333 SIXTEENTH ST. N. W., WASHINGTON 6, D. C.

\$2.00 volume

\$1.50 issue

Entered as second-class matter July 14, 1909, at the post office at Lancaster, Pa., under Act of Congress of March 3, 1879.

Acceptance for mailing at the special rate of postage provided for in paragraph (654), Section 2642, P. L. & C. of 1949, authorized Jan. 4, 1949.

Send all correspondence, including address changes, to 1333 Sixteenth St. N.W., Washington 6, D. C. Address changes must arrive by the 15th of the month to take effect the following month. Undelivered copies resulting from address changes will not be returned; subscribers should notify the post office that they will guarantee second-class forwarding postage. Other claims for undelivered copies must be made within four months of publication.

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THE PSYCHOLOGICAL REVIEW

THE EFFECTS OF EARLY HANDLING ON VIABILITY OF THE ALBINO RAT¹

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This paper concerns effects of early handling on viability of the albino rat. The relation of early experience, particularly handling, to the response to stress will be considered first; then neurophysiological mechanisms mediating the observed effects of handling on viability of the rat will be explored.

Experiments by Selye (60, 61) in 1936 showed that a wide variety of noxious agents and stimuli—e.g., poisons, ionizing radiation, extreme cold, excessive exercise, surgical injury, and emotional excitement—produce a non-specific, systemic response in the vertebrate organism. This adaptive response involves release of adrenocorticotrophic hormone (ACTH) from the anterior pituitary, and consequent release of adrenal cortical hormones, such as corti-

sone and 17-hydroxycorticosterone, into the blood stream. The general effect of these adrenocortical hormones is to maintain blood pressure and blood-sugar levels, thus counteracting shock effects in injury and trauma. These hormones stimulate metabolism at the cellular level² and, in general, their end-effects are protein catabolic, involving breakdown of protein into glucose.

There appears to be general agreement in 1958 that almost any type of disturbing stimulus induces a pituitary-adrenal response to stress in the rat, and that this reaction is essential to preserving the animal's metabolic and circulatory integrity under stress, as Engel stated in 1953 (14).

The work of Selye (60) at Montreal has indicated that prolonged stress, like cold or excessive exercise, will lead to degenerative renal and cardiovascular changes in the rat and, if sufficiently prolonged, to the subsequent death of the animal. A number of studies in the

¹ This material is based, in part, on a report to the National Research Council of Canada, Jan. 3, 1953, and on a paper subsequently presented to the Canadian Psychological Association, May 28, 1953, at Kingston, Ontario. Related research at the University of Toronto was made possible by Grant A.P. 20 from the National Research Council of Canada to the writer, 1952-1954, and by the friendly and continued support of E. A. Bott, Head, Department of Psychology, University of Toronto. Preparation of this paper was made possible by a grant from the Research in Humanities Fund of the University of Toronto.

² A stress not encountered in nature is oxygen at high pressure. Under this stress, presence of the adrenals is detrimental to survival of the rat (17). These and similar experiments (3, 16) suggest that the normal action of the adrenal cortex is to enhance metabolic processes, particularly oxidation, at the cellular level.

United States have, however, suggested that there are marked effects of early individual attention, including handling and petting, on growth and survival of the albino rat.

Hammett (23) in 1922 found that rats stroked or petted were much more likely to survive an operation for removal of the parathyroid gland than those that had not been handled. Bernstein (4) reported in 1952 that handled rats gained more weight than unhandled controls, and also learned mazes faster and with fewer errors.

These and other considerations suggested the hypothesis that early-handled rats would be better able to withstand severe stress than nonhandled controls. This hypothesis was tested in a series of experiments at the University of Toronto under the writer's direction.

Handling and viability. This research has shown that early handling of the albino rat increases viability under stress as an adult (7). Weininger, in two independent experiments (68, 69), found that early-handled rats, subjected to 48 hours of immobilization and food and water deprivation as adults, sustained less heart and gastrointestinal damage than unhandled controls. Long (7) found that early-handled rats survived complete food and water deprivation as adults 53.8 hours longer than matched controls, thus confirming the results of an earlier pilot study (67) by Weininger. In general, early-handled animals showed less emotional reactivity to a strange environment, and their adrenals did not become hyperreactive under stress (69). These results suggest that early handling raises the threshold for the pituitary-adrenal response to stress.

In the Toronto studies, early handling involved holding the rat loosely in the left hand against the experimenter's chest and stroking the rat "with the grain," head to tail, for 10 minutes a

day, a slight modification of the method originally developed by Bernstein (5). For convenience through the remainder of this paper, this general method will be referred to as "Type I" handling. In the Toronto studies, handling was carried on for 21 days after weaning (age 21 days).

These findings have been in part confirmed by subsequent experimental work in the United States. Levine and Otis (36) at Ohio State found that rats handled prior to weaning showed significantly less mortality following food and water deprivation than nonhandled rats or rats handled after weaning. Levine (32) has further shown that early-handled mice have a higher threshold for emotional reactivity than nonhandled controls. Levine has also found recently (34) that adrenal weights of rats handled prior to weaning were significantly lower, following the stress of glucose injection, than those of controls. In the latter study, he found no significant difference in mortality under this stress between early-handled and control rats.

In these studies of Levine and his colleagues, however, early handling involved removing the infant rat from its nest, placing it in a different environment (as on a scale or in a box) and then returning it to its nest.³ In the Levine and Otis study, for example, this picking-up-and-returning procedure probably involved no more than a minute all told, and probably no more than 12 seconds' contact with the experimenter.⁴ Yet this abbreviated pro-

³ In a study at Toronto, Newton (7) found that prolonged (postweaning) experience with the mother rat increased the infant rat's emotional reactivity as an adult, compared to handled rats, and that this extra mothering lowered the basal metabolic rate and weight for the rat, compared to controls that had not been either extra-mothered or handled.

⁴ Personal communication with S. Levine, 1957.

cedure was found to have the same protective effects under stress⁵ as the Toronto method, also used by Levine and Otis in their study (36). For the sake of convenience, the kind of handling used by Levine will be referred to as "Type II" handling in this paper. It should be noted that in the Levine studies "early handling" refers in general to handling before weaning.

In a replication of Weininger's experiments (68, 69), but using group-housed rather than individually-caged rats as in the Toronto studies, Stern and Taylor⁶ have recently found significantly reduced gastrointestinal lesions in early-handled (Type I) rats following 48 hours of immobilization, compared to controls, but no differences in adrenal weight, heart damage, or emotional reactivity under these conditions.

An experiment by Scott (59), contrary to the findings of Weininger and Levine, showed no differences in weight, emotional reactivity, or survival under severe physical stress (massive doses of thiourea) between early-handled (Type I) and control rats. In this experiment, however, a severe stress in the form of a heat wave intervened between the early handling and the experimental tests. The effects of such intervention of stress on the growth gains usually induced by early handling are discussed below.

A number of recent studies have suggested that sensory input from other modalities may influence the effects of handling on viability. An experiment

by King⁷ has shown that even visual contact with others of its species has survival value for the guinea pig in infancy, when the animal is already under dietary stress (vitamin deprivation). An experiment by Shelly, Pasamanick, and Levine (62) showed that rats held loosely in a black box a few minutes a day for nearly three weeks evidenced only transient growth gains over controls during the next week, irrespective of their age at time of handling. Rats handled prior to weaning in this experiment also showed a decrease in timidity in a strange situation that disappeared after one trial. Macdonald and Teghtsoonian (38) found that three minutes of intense light stimulation produced a transient but significant weight gain in rats that had previously received maximum visual stimulation in infancy, compared to controls that also experienced the intense light as adults but that had been subjected to less visual stimulation in infancy.

These results suggest, as Thompson has hypothesized (66), that the total quantity of sensory interaction with the environment, rather than the specific nature of the sensory input, is the essential factor involved in effects of early experience on viability. If this hypothesis were correct, then the effects of early handling on viability would depend on the amount of visual stimulation concomitantly experienced by the young animal. It might be expected, in this case, that rats handled in the dark would be less resistant to stress as adults than those handled in daylight or under artificial light.

BASIC HYPOTHESES

Stress threshold. The key hypothesis of the position presented here is that early handling raises the threshold for response to stress, thus conserving the

⁵ In his experiment at Toronto, mentioned above, Long (7) found that rats that were merely picked up and held against the experimenter's chest for 30 seconds, and then returned to their cages, survived longer under total food and water deprivation than controls, but this difference was not statistically significant.

⁶ Unpublished study by J. A. Stern and R. Taylor, School of Medicine, Washington University, 1957.

⁷ Personal communication with J. A. King, 1957.

metabolic resources or adaptation energy (60) of the organism.⁸ The response to stress may be differentiated into two reactions. Stimulation of the adrenal medulla by the sympathetic division of the autonomic nervous system results in release of epinephrine and nor-epinephrine into the blood stream. And, as noted above, stimulating of the adrenal cortex by ACTH from the anterior pituitary results in release of adrenocortical hormones, such as cortisone and 17-hydroxycorticosterone, into the blood stream. Both these reactions are, in general, mediated by the hypothalamus (25) and they may be considered, as Cleghorn (9) suggests, to be the two arms of a single response to

⁸ Conversely, this suggests that an animal with a low stress threshold would, through leakage of its reserves (which can be conceived of as the organism's neuronal, hormonal and metabolic resources) across this threshold, lose its capacity to deal with the major stresses of later life. That is to say, *early stress lowers resistance to later*. Relevant here is the finding of Jones (28, 29) that a major factor in the death rate of normal human populations from such diseases as cancer is the amount of childhood disease experienced. Early stress, including disease, can be thought of as chipping away at the body's defenses and lowering resistance to later major and lethal stress (28). On the other hand, Levine (31, 35) has hypothesized that early handling constitutes a stressful situation for the infant organism, and that such early (minimal) experience with noxious stimulation results in a greater ability of the organism to adapt to psychological and physiological stress as an adult. He has recently found, for example, that rats shocked and handled in infancy, or handled alone (the handling consisting of no more than 12 sec. of contact with the experimenter per day), showed superior weight gains and less emotionality compared to control rats that had received neither handling nor shock in infancy (33). Thus, *either* early stress lowers resistance to later, as is hypothesized here, *or* early stress enhances resistance to later stress, as Levine has suggested. This point should be experimentally decidable, using such sensitive indices of resistance to stress as the total lifespan of the animal.

noxious stimuli. It can further be suggested, with Sayers (55), that both arms of the response to stress usually occur together.

While the work of Selye has emphasized the pituitary-adrenal response in general in accounting for stress damage, recent investigations by Goldstein (21) and others, to be considered below, have pointed to the crucial role of the sympathetic system in tissue damage.

The basic hypothesis of this paper may therefore be restated as follows: *early handling of the albino rat reduces (a) the pituitary-adrenal and (b) the sympathetico-adrenal medulla responses to later stress.*

The evidence for each of the two sections of this hypothesis will now be considered in turn, and further relevant experiments suggested in each case.

A reduced *pituitary-adrenal output* in early-handled animals under stress is suggested by failure of their adrenals to show hyperactivity under these conditions. In two experiments, Weininger (69) found significantly lower adrenal weights in early-handled (Type I) rats than in unhandled ones, following 48 hours of immobilization and food and water deprivation. Levine (34), using a different method of handling (Type II), as noted above, found significantly lower adrenal weights in rats handled prior to weaning than in unhandled ones, following the stress of glucose injection.

A more conclusive experiment would involve comparison of the adrenal-steroid (17-hydroxycorticosteroid) blood-plasma content of early-handled and of control animals under stress, after the method of Nelson and Samuels (44). Also pertinent would be comparison of resting 17-hydroxycorticosteroid levels in early-handled and in control animals, as a further check on the hypothesis of reduced pituitary-adrenal activity in the former. A third, and critical, experiment would involve measurement of

ACTH blood content in early-handled and unhandled adrenalectomized animals under stress, using the method of Sydnor and Sayers (65). The animal of choice for these experiments might be the guinea pig, since the resting level of 17-hydroxycorticosteroid blood plasma content in the rat is so low as to be virtually undetectable (12).

Even if it were further confirmed, however, through the experiments suggested above, that early handling reduces pituitary-adrenal output, this would by no means demonstrate that such reduction was due to a dampening of pituitary-adrenal activity. It could equally well be due to a greater capacity of the pituitary-adrenal axis in early-handled animals for the normal production of adrenal cortical steroids, as Levine has suggested (35), so that for this reason there would be less detectable output under stress.

Levine (35) has recently shown that handling (Type II) after birth induces significant increases in ascorbic acid depletion (an index of stress response) in rat adrenals under stress, and on the basis of this and other work concludes that handling accelerates maturation of the pituitary-adrenal system in the rat. In this connection, Woods (72) has shown that the wild Norway rat, with markedly greater adrenal capacity than the laboratory rat, shows no detectable adrenal ascorbic-acid depletion as an adult under such stress as low temperature, fighting, or loud noise. In this case, then, the greater capacity of the wild rat results in a reduced pituitary-adrenal output under stress, compared to the laboratory animal.

If the hypothesis that early-handled rats had, as a result of handling, greater adrenal capacity than unhandled controls, were correct, several experimentally testable hypotheses should follow.

First, this greater capacity should be

evidenced in greater weight of the adrenals under normal conditions, since the greater capacity of the wild rat is reflected in adrenals proportionately two to four times the size of those of laboratory rats, the difference being in size of the adrenal cortex (50). No differences between adrenal weights of early-handled (Type I) and unhandled rats have, however, been detected prior to stress (69).

Second, this presumed greater capacity and greater normal output should be reflected in a higher adrenal-steroid (17-hydroxycorticosteroid) resting level in handled, compared to unhandled animals. But this would be precisely the reverse of the expectation if the position advanced in this paper were true, for if anterior pituitary and adrenocortical activity were permanently dampened under early handling, then the resting 17-hydroxycorticosteroid level should be *lower* than that in unhandled controls.

The question thus resolves itself into whether the obtained reduction in pituitary-adrenal output under stress of early-handled animals, as found by Weininger (69) and Levine (34), is due to a concomitant reduction in stimulation of the anterior pituitary and hence in ACTH output, as suggested in this paper, or is due to accelerated maturation and consequently greater capacity of the pituitary-adrenal system in early-handled animals, as suggested by the work of Levine (35).

A reduction of *sympathetic-adrenal medulla response* in early-handled animals under stress is suggested by the reduced emotional reactivity that has usually, but not always, been found in these animals as adults.

Weininger (69) found that early-handled (Type I) rats, introduced to a circular open field enclosed by three-foot metal walls with a 200-watt bulb hung over the center, ventured further to

wards the center of this arrangement than controls (two experiments), and showed more activity than controls (two of three experiments, the result being reversed in the third). Levine (32) found that early-handled (Type II) mice showed less latency in leaving their cages for food and in fighting other mice than controls. Bernstein (5) found that rats handled (Type I) prior to and during the course of the experiment showed more vigorous exploratory behavior in a maze and in their home cages than did controls, learning mazes faster and with fewer errors. Reduced emotional reactivity of handled rats could account for both their superior maze performance and greater exploratory activity.

In a recent experiment, Rosen (51) found early-handled (Type I) rats dominant in paired food competition with unhandled controls, and suggests reduced emotional reactivity of handled rats as the relevant factor. No correlation was found between weight and dominance.

On the other hand, Gertz (18) found in a recent study that early handling (Type I) of the albino rat had no effect on adult emotional behavior, as measured by frequency of defecation, number of boluses excreted, and activity in an open field.

The basic hypothesis of reduced sympathetic-adrenal medulla response of early-handled animals under stress could be tested by comparing cardiac rate and peripheral sympathetic reaction (blood pressure in the tail, for example) of early-handled and control rats under stress. It could further be tested by comparing blood and urine output of epinephrine from the adrenal medulla of early-handled and control rats subjected to noxious stimuli.

Although a number of interesting clean-up studies remain to be done in this area, it is the conviction of the

writer that the point of diminishing returns has been reached in current psychological studies of the effects of the early experience of handling on the response to stress, and that the time is now ripe to enquire into the underlying neurophysiological mechanisms operating to produce the observed results.

HYPOTHALAMIC REACTIVITY

If the effects of early experience of handling on viability are to be accounted for in terms of the threshold for stress, then the effects of such experience on the neurophysiological determinants of the stress response must be shown. The writer (6) has suggested that early handling dampens posterior hypothalamic reactivity to stress. Porter (45, 46) has shown that this is the area (mammillary body and median eminence) involved in the pituitary-adrenal response to stress. Such dampening of the posterior hypothalamus would inhibit the sympathetic-adrenal medulla stress response as well as the pituitary-adrenal cortex reaction, since both of these "arms" of the response to stress are mediated by the posterior hypothalamus (25, 42, 46).

Sympathetic response. Both of these arms usually occur together, as Sayers (55) has pointed out. Virtually all conditions that release ACTH and are considered stress situations also increase sympathetic nervous activity and the secretion of epinephrine.⁹

In accounting for stress damage to the cardiovascular and renal systems, the work of Selye (60) has put the onus on the pituitary-adrenal response and the consequent production of adrenal cortical steroids. Recent investigations by Goldstein (21) and others have, however, shifted emphasis to the sympathetic division of the autonomic sys-

⁹ Personal communication with M. S. Goldstein, 1956.

tem as the primary agent in tissue damage under stress. For example, adrenalectomized dogs maintain a precarious balance under severe physical stress to which they would otherwise succumb, providing they have been sympathectomized or have been treated with drugs that block action of the sympathetic system (21).⁹ This work of Goldstein and others suggests that adrenal cortical hormones have the function of buffering the tissues against damage from epinephrine and nor-epinephrine.

Without such preparation of minute blood vessels by adrenal steroid hormones, autonomic neurohumoral activity appears to result in irreversible damage, and hence probably in the terminal conditions of local haemorrhage and decreased blood pressure (21). On the other hand, interruption of the sympathetic system protects the adrenalectomized animal from stress.⁹ Goldstein notes in this connection: "It is as if the price paid for a highly reactive system such as the sympathetic is to expose the tissue to damage with the steroids functioning to support the tissue in meeting the metabolic stimulation of the adrenals."⁹

Other experiments have shown the protective effects of sympathetic blocking agents in tumbling shock (52) and in tourniquet and burn shock (40). It may therefore be suggested here that the protective effects of early handling result from just such a dampening of sympathetic activity under stress. This hypothesis could be directly tested by comparison of the relative protective effects of early handling and of sympathetic blocking agents for rats under severe physical stress, such as tumbling shock. The hypothesis could be indirectly tested by comparing the startle and peripheral sympathetic response (as in tail blood pressure) to sudden stimuli of early-handled rats and controls.

Hypothalamic balance. The major

mechanism here suggested to account for the effects of early handling on viability remains, however, dampening of posterior hypothalamic reactivity (6). Inhibition of the sympathetic autonomic response, and of the pituitary-adrenal response, would be subsidiary effects of this change in hypothalamic function. This hypothesis could be tested directly by comparing electrical activity in the posterior hypothalamus of early-handled and control rats under physical, physiological, and emotional stress. For example, the electrical response to epinephrine injection could be compared in handled and control animals.

The neurophysiological mechanisms involved in such postulated posterior hypothalamic dampening have not been investigated.

Recent experimental work by Redgate and Gellhorn (15, 49) suggests, however, that balance of excitability between the anterior and posterior hypothalamus determines pulse-rate and blood-pressure levels. Coagulation or drug-induced reduction of posterior hypothalamic excitability resulted in a reduction of blood pressure and pulse rate. Coagulation of the anterior hypothalamus resulted in opposite changes.

Other research by Shimazu et al. (63) in Japan has shown that stimulation of parasympathetic hypothalamic centers in the rabbit inhibits secretion in the posterior hypothalamus of the neurosecretory granules¹⁰ involved in the response to stress. Their findings indicate that the neurosecretory material is released into the hypothalamic and hypophyseal blood vessels by stimulation of the posterior sympathetic centers of the hypothalamus, or by such stresses as wounds, burns, or poison.

¹⁰ The neurosecretory cells, which are equipped to transform neural impulses into humoral activity, thus provide the mechanism for linking the two great integrating systems of the body (56).

The work of Scharrer (56, 57) and Harris (25) has indicated that it is the transmission of this material from the posterior hypothalamus to the anterior pituitary through the portal blood vessels that initiates the release of ACTH from the pituitary and thus triggers the pituitary-adrenal response to stress, particularly when the noxious stimulus is mediated by the central nervous system (as is the case for emotional stress). The important finding of the Japanese investigators, for the present purpose, is that stimulation of the parasympathetic centers suppresses the release of this neurosecretory material and causes its retention in the axon.

Those environmental stimuli that activate anterior and parasympathetic hypothalamic centers would, therefore, at the same time inhibit the posterior hypothalamic nuclei that mediate the two arms of the response to stress. The balance between activity of the anterior and posterior hypothalamus can thus be termed the threshold for stress. The more dominant the posterior hypothalamic centers over the anterior, the greater the probability that a neutral stimulus would result in a stress response, and thus the lower the threshold for this response.

To account for the effects of early handling on viability of the albino rat, it can now be suggested that early handling stimulates the anterior hypothalamus and parasympathetic centers and thus inhibits the posterior hypothalamic response to stress. If this sensory input from handling occurs early enough in the development of the organism, then the balance is permanently shifted in favor of the anterior region.

The relative dominance of one or the other of these two response systems, determined genetically or by early experience, can thus be considered the threshold for stress. That is to say, the probability that the rat will respond to a

neutral environmental stimulus with a protein catabolic reaction is, in part, determined by the rat's early sample of what life is like for rats.

The central proposition of this paper is, therefore, that early handling alters the balance of hypothalamic excitability in favor of the anterior (and parasympathetic) regions, thus having the joint effects of (a) reducing pituitary-adrenal and sympathetic-adrenal medulla responses under stress by the mechanism noted above, and (b) stimulating anabolic processes, such as an increase in secretion of growth hormone from the anterior pituitary (handling has been found to increase weight and skeletal length of rats in a number of experiments, noted below), through a mechanism not yet determined.

The anticatabolic effects of early handling in reducing pituitary-adrenal and sympathetic-adrenal medulla responses under stress have been considered above. The anabolic effects of early handling will now be considered in some detail.

Anabolic effects. In respect to anabolic stimulation from early handling, the finding of Doolittle and Meade (13) that free amino-acid blood-serum content of early- and late-handled (Type I) rats shows an acute reduction, compared to unhandled controls, suggests that protein synthesis is increased, and/or protein breakdown reduced, in handled animals. This indirectly supports the hypothesis that early handling stimulates growth hormone secretion, as suggested by Harris (24).

In this connection, Mogenson et al. (41) have found that the weight gain usually induced by early handling (Type I) is nullified either by injection of cortisone (an adrenal cortical hormone) or by the stress of a high-frequency sound, both effects presumably operating to counteract growth hormone or reduce its output. Ruegamer and Silverman (54) have shown that rats han-

dled (Type I) two weeks preceding electric shock, and with resultant gains in growth and food utilization over controls, lost these advantages when both groups were subjected to electric shock. Both groups were inferior to nonshocked controls.

This effect of stress on anabolic effects of early handling might account for the failure to obtain weight gains from handling (Type I) in the experiments of Newton (7), Long (7),¹¹ and Scott (59). The heat wave that intervened in the latter experiment may have served to wipe out any differences between experimental and control groups. The Newton and Long results may have been related to the exposure of their rats to cold in transit from upper New York State to Toronto in the winter, but this is only speculation. In any case, a majority of studies in the literature¹² have shown that early handling induces weight gain in the rat, and Ruegamer et al. (53) have shown this gain (from Type I handling) to be the result of better food assimilation rather than of greater consumption. This again

suggests the influence of growth hormone (somatotrophin or STH).

How increased growth-hormone output could lead to enhanced survival under such specific and severe types of stress as complete food and water deprivation, as found for early-handled rats in the studies of Long (7) and Levine and Otis (36), is suggested by the relation of STH to cardiac glycogen deposition.

Studies of Adrouny and Russell have shown, respectively, that STH increases cardiac glycogen under normal conditions and that it has an important role in maintenance of cardiac glycogen stores during the emergency of fasting (1, 2). The greater survival of early-handled (Type I) adult rats (age 52 days) under food and water deprivation than of controls of the same weight, as found by Long (7),¹¹ is otherwise rather difficult to explain; but it could be accounted for on the assumption that these animals were able to maintain cardiac glycogen (a form of carbohydrate) stores longer than controls. This hypothesis could be experimentally checked by comparison of cardiac glycogen deposits in early-handled and control rats (a) under food and water deprivation and (b) under normal conditions.

If exploration of the hypothesis that early handling alters the balance of hypothalamic activity in favor of the anterior region should prove fruitful, the next logical step would be investigation of the neurophysiological determinants of such altered neuroendocrine response.

This would require, in the first instance, recording of anterior and posterior hypothalamic activity from chronically implanted electrodes, to test the specific hypothesis of anterior dominance. The specific hypothesis to be tested here would be that, in early-handled animals as compared to controls,

¹¹ Personal communication with H. G. Long, 1955.

¹² These include an original experiment by Bernstein (4), three subsequent experiments by Weininger (69, 70), three by Ruegamer et al. (53), and one by Brooker (8), all involving Type I handling. Levine found weight gains from handling (Type II) in his glucose-injection experiment (34). Both Weininger (69) and Ruegamer (53) found that early handling (Type I) increased skeletal length. In another experiment in this area, McClelland (37) found that stroking the rat (in a restraining box) with an artist's brush for the same period had exactly the same effect in inducing weight gain as personal (Type I) handling by the experimenter. This latter finding supports Weininger's view (67, 70) that the essential variable involved in handling is tactile stimulation, rather than supporting Bernstein's view (5), suggested to account for his findings on handling (Type I) and maze learning, that the essential element in handling is relationship of the animal to the experimenter.

(a) posterior hypothalamic excitability would be reduced, and (b) anterior hypothalamic excitability would be increased, under both normal and stress conditions.

The hypothesis of anterior dominance could be indirectly tested by comparing pulse rate and blood pressure of early-handled and control rats, under both normal and stress conditions. The work of Redgate and Gellhorn (49), discussed above, suggests that if the anterior hypothalamus is dominant in early-handled rats, as hypothesized, these animals should evidence, compared to controls, (a) a slower return of blood pressure from drug-induced hypotension, and (b) an increase in reflex slowing of the pulse from nor-epinephrine. The most conclusive experiment in respect to anterior dominance would, however (as suggested above), involve application of a stress agent to the organism (epinephrine, for example) and consequent recording of hypothalamic electrical activity, comparing early-handled and control rats in this respect.

MODULATION OF HYPOTHALAMUS

Amygdaloid complex. If a change in the balance of hypothalamic activity is suggested to account for effects of early handling on viability, then the question arises as to just how such a change could be effected. Recent studies by Gloor (19, 20) and Pribram (47) of the amygdaloid complex¹³ and its diffuse projection system to subcortical regions suggest that one of its functions is

to modulate hypothalamic excitability. Other research by Schreiner and Kling (58) has shown that removal of the amygdaloid complex reduces behavior of both savage and domestic animals to tameness and docility. The primitive function of the olfactory cortex, of which the amygdaloid complex is an element, can be thought of, after Herrick (26), as qualitative discrimination between desirable and noxious stimuli and consequent nonspecific arousal of the appropriate subcortical centers, particularly hypothalamic.

If the function of the amygdaloid complex includes discrimination between desirable and noxious stimuli, then its removal should interfere with such discrimination. Relevant here is the finding of Weiskrantz (71) that the effect of amygdectomy on monkeys is to make it difficult for the animals to identify motivationally relevant stimuli (in the words of J. Olds), whether desirable or noxious.

A study by Lesse (30) has shown that stimulus situations involving behavioral responses of agitation, escape, attack, or defense induce in the cat a distinct electrographic pattern (high amplitude, 40-45 sec. frequency) in the lateral nuclei of the amygdaloid complex.

Since these behavioral responses are precisely those that Hess (27) has obtained from direct electrical stimulation of hypothalamic centers (also in the cat), a connection between amygdaloid and hypothalamic response to stress would seem indicated here. It should also be noted from the Lesse study that stimuli which resulted in waking or alerting responses only, and which have been found in other studies to produce large-amplitude, synchronized waves in the hippocampus (22), did not induce the amygdaloid EEG pattern typically elicited by noxious stimuli, such as electric shock.

Indirect evidence for amygdaloid

¹³ The amygdaloid nucleus is a complex gray mass, covered by a layer of rudimentary cortex, in the dorsomedial region of the temporal lobe, near the tip of and on the floor of the inferior horn of the lateral ventricle. While it is considered to belong to the olfactory secondary centers, it has a good many nonolfactory connections, particularly somesthetic (39), and may be thought of as an olfactory thalamus or integrating center. Cf. Strong and Elwyn (64), Ranson and Clark (48).

modulation of hypothalamic reactivity comes from de Florida and Delgado's finding (10) that stimulation of the amygdaloid nucleus induces affectionate and playful behavior in a group of male cats, with effects lasting for several days. An experiment of Morgane and Kosman (43), however, contrary to the trend of the above studies, suggests that the amygdaloid complex has no effect on hypothalamic reactivity. They found that bilateral amygdectomy in cats produced no observable changes in affective behavior, including reaction to noxious stimuli or petting.

The question remains as to how the sensory input from early handling could affect amygdaloid activity. Relevant in this connection is the study of Machne and Segundo (39), who found stimulation of the sciatic nerve and touching of any portion of the skin most effective in arousal of electrical activity in the amygdaloid nucleus. From this study, olfacto-somatic correlation appears to be a major function of this subcortical center.

The above work, taken together, suggests that the balance of hypothalamic activity, and therefore the threshold for stress, may be determined by amygdaloid mediation. The testing of this hypothesis would require removal of the amygdaloid complex by suction in experimental and matched control animals before handling, to determine whether the effects of handling on viability would thereupon be eliminated. Further, electrical recordings of amygdaloid activity during stress and under normal conditions could be made to determine any differences between early-handled and control animals.

Hippocampus. The role of the hippocampus (see Fig. 1) in modulating hypothalamic activity also requires exploration. Relevant in this respect are (a) the finding of Gloor (19) that hippocampal responses to electrical stimu-

lation of the amygdaloid nucleus reach the largest amplitudes of any structure fired by the amygdala; and (b) a reported finding of J. W. Mason¹⁴ that electrical stimulation of the hippocampus itself acts to suppress the pituitary-adrenal response to stress.

These findings would suggest, then, that the amygdaloid complex may have at least two pathways for hypothalamic modulation—a direct projection to the anterior hypothalamus via the stria terminalis, with direct fiber connections (19), and an indirect, multisynaptic pathway to the hippocampus, whose main efferent projection (the fornix) is to the posterior hypothalamus, in the region of the mammillary body (see Fig. 1). This is, of course, precisely the region found by Porter (45, 46) to mediate the pituitary-adrenal response to stress.

Delgado (11) found defensive reactions in monkeys following electrical stimulation of the hippocampus and fornix, and, on the basis of his own and other work, suggests the hippocampus as locus for nociceptive sensory integrations. These and other investigations impute to the hippocampus a leading role in the mediation of noxious stimuli and hence in the neuroendocrine response to stress, although no simple or clear-cut picture of hippocampal function emerges from this research.

SUMMARY

The central proposition of this paper is, therefore, that effects of early handling on viability of the albino rat are mediated by a permanent alteration in the balance of hypothalamic activity, in favor of the anterior region. This alteration results in increased growth hormone output under normal conditions, and decreased activity of the pituitary-

¹⁴ Personal communication with J. V. Brady, 1957.

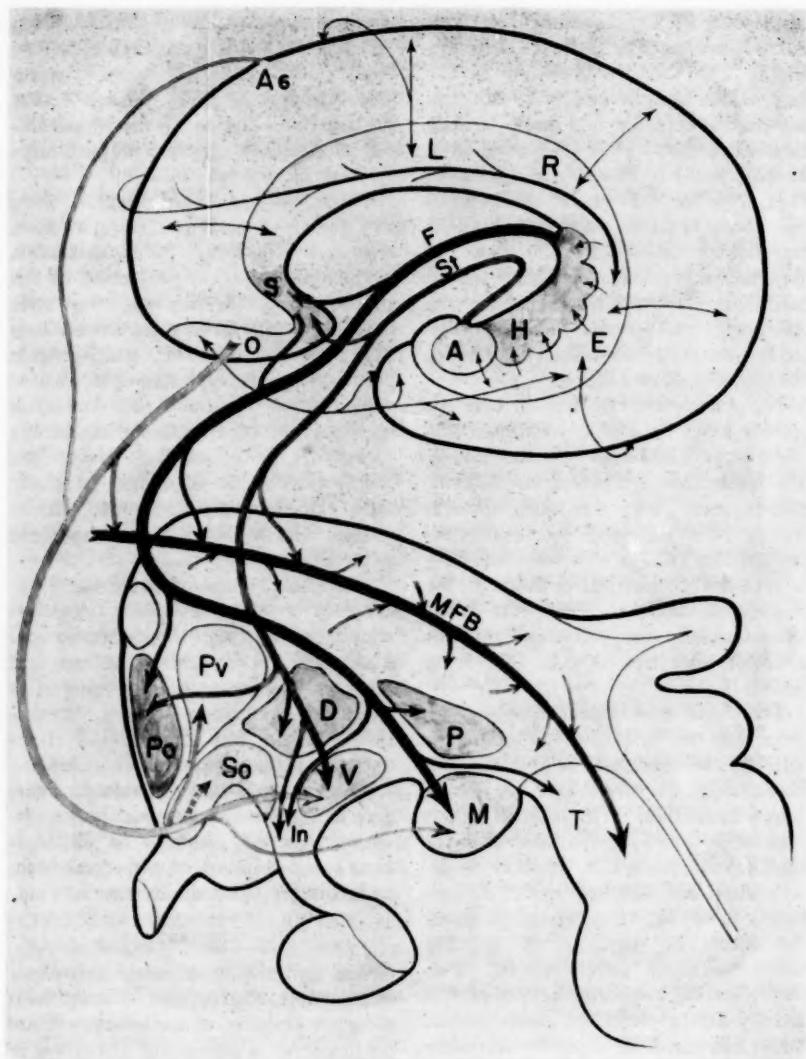


FIG. 1. Diagrammatic representation of the main telencephalo-hypothalamic connections. The arrows indicate the direction of nervous impulses in this system. The arrows in the diagram of the hemisphere outline the main associational connections at the cortical level, which are relevant for this system. (From Gloor, P. Telencephalic influences upon the hypothalamus. In W. S. Fields, R. Guillemin, & C. A. Carton (Eds.), *Hypothalamic-hypophyseal interrelationships*. Springfield, Ill.: Charles C Thomas, 1956. Pp. 74-113. Courtesy of the author and Charles C Thomas, Publisher.)

A: amygdala. A6: Area 6 of frontal lobe.
D: dorsomedial nucleus of hypothalamus.

E: entorhinal area (pyriform lobe).
F: fornix.

adrenal cortex and sympathetico-adrenal medulla systems under both normal and stress conditions. In turn, this alteration in hypothalamic balance is itself the result of a change in amygdaloid complex activity, arising from the sensory input from early handling.

Experimental tests of derivative hypotheses have been suggested in the course of this presentation.

REFERENCES

- ADROUNY, G. A., & RUSSELL, JANE A. Growth hormone and myocardial glycogen. *Fed. Proc.*, 1954, **13**, 1. (Abstract)
- ADROUNY, G. A., & RUSSELL, JANE A. Effects of growth hormone and nutritional status on cardiac glycogen in the rat. *Endocrinology*, 1956, **59**, 241-251.
- BEAN, J. W. Reserpine, chlorpromazine and the hypothalamus in reactions to oxygen at high pressure. *Amer. J. Physiol.*, 1956, **187**, 389-394.
- BERNSTEIN, L. A note on Christie's "Experimental naïveté and experiential naïveté." *Psychol. Bull.*, 1952, **49**, 38-40.
- BERNSTEIN, L. The effects of variations in handling upon learning and retention. *J. comp. physiol. Psychol.*, 1957, **50**, 162-167.
- BOVARD, E. W. A theory to account for the effects of early handling on viability of the albino rat. *Science*, 1954, **120**, 187.
- BOVARD, E. W., & NEWTON, D. G. Systematic early handling and prolonged experience with the mother as developmental variables in the male albino rat. In N. R. Brewer (Ed.), *Proceedings of animal care panel*, 1955. Chicago: Animal Care Panel, 1956. Pp. 67-74.
- BROOKER, H. The effect of the amount of early handling on weight gain in the albino rat. Unpublished master's thesis, Univer. of Toronto, 1955.
- CLEGHORN, R. A. Discussion of F. L. Engel, General concepts of adrenocortical function in relation to the response to stress. *Psychosom. Med.*, 1953, **15**, 571-573.
- DE FLORIDA, F. A., & DELGADO, J. M. R. Lasting effects on behavior evoked by cerebral stimulation of the cat. *Amer. J. Physiol.*, 1955, **183**, 592.
- DELGADO, J. M. R. Cerebral structures involved in transmission and elaboration of noxious stimulation. *J. Neurophysiol.*, 1955, **18**, 261-275.
- DONE, A. K., ELY, R. S., RAILE, R. B., & KELLEY, V. C. Species differences in circulating 17-hydroxycorticosteroid concentrations. *Proc. Soc. exp. Biol., N. Y.*, 1952, **81**, 667-669.
- DOOLITTLE, R. F., & MEADE, R. The effect of gentling on some psychological and physiological phenomena. Paper read at Eastern Psychological Association, New York, April 13, 1957.
- ENGEL, F. L. General concepts of adrenocortical function in relation to the response to stress. *Psychosom. Med.*, 1953, **15**, 565-573.
- GELLHORN, E. *Autonomic imbalance and the hypothalamus*. Minneapolis: Univer. Minnesota Press, 1957.
- GERSCHMAN, REBECCA, GILBERT, D. L., NYE, S. W., NADIG, P. W., & FENN, W. O. Role of adrenalectomy and adrenal-cortical hormones in oxygen poisoning. *Amer. J. Physiol.*, 1954, **178**, 346-350.
- GERSCHMAN, REBECCA, & NADIG, P. W. Stress and oxygen poisoning. *Fed. Proc.*, 1953, **12**, 50-51. (Abstract)
- GERTZ, B. The effect of handling at various age levels on emotional behavior of adult rats. *J. comp. physiol. Psychol.*, 1957, **50**, 613-616.
- GLOOR, P. Electrophysiological studies on the connections of the amygdaloid nucleus in the cat. Part I: The neuronal organization of the amygdaloid projection system. *EEG clin. Neurophysiol.*, 1955, **7**, 223-242.
- GLOOR, P. Electrophysiological studies on the connections of the amygdaloid nucleus in the cat. Part II: The electrophysiological properties of the amygdaloid nucleus.

H: hippocampus.

In: infundibulum.

L: limbic (cingulate) gyrus.

M: mamillary body.

MFB: medial forebrain bundle.

O: orbitofrontal cortex.

P: posterior hypothalamus.

Po: preoptic region.

Pv: paraventricular nucleus.

R: retrosplenial and posterior cingulate region.

S: septal area (subcallosal region).

So: supraoptic nucleus.

St: stria terminalis.

T: tegmentum.

V: ventromedial nucleus.

- daloid projection system. *EEG clin. Neurophysiol.*, 1955, **7**, 243-264.
21. GOLDSTEIN, M. S., RAMEY, E. R., FRITZ, I., & LEVINE, R. Reversal of effects of stress in adrenalectomized animals by autonomic blocking agents. *Amer. J. Physiol.*, 1952, **171**, 92-99.
 22. GREEN, J. D., & ARDUINI, A. A. Hippocampal electrical activity in arousal. *J. Neurophysiol.*, 1954, **17**, 533-557.
 23. HAMMETT, F. S. Studies of the thyroid apparatus. *Endocrinology*, 1922, **4**, 221-229.
 24. HARRIS, G. W. *Neural control of the pituitary gland*. London: Arnold, 1955.
 25. HARRIS, G. W. Hypothalamic control of the anterior lobe of the hypophysis. In W. S. Fields, R. Guillemin, & C. A. Carton (Eds.), *Hypothalamic-hypophyseal interrelationships*. Springfield, Ill.: Charles C Thomas, 1956. Pp. 31-42.
 26. HERRICK, C. J. The function of the olfactory parts of the cerebral cortex. *Proc. nat. Acad. Sci., Wash.*, 1933, **19**, 7-14.
 27. HESS, W. R. *Diencephalon. Autonomic and extrapyramidal functions*. New York: Grune & Stratton, 1954.
 28. JONES, H. B. Demographic consideration of the cancer problem. *Trans. N. Y. Acad. Sci.*, 1956, Series II, **18**, No. 4, 298-333.
 29. JONES, H. B. A special consideration of aging process, disease and life expectancy. In J. H. Lawrence and C. A. Tobias (Eds.), *Advances in biological and medical physics*, 4. New York: Academic Press, 1956. Pp. 281-337.
 30. LESSE, H. Electrographic recordings of amygdaloid activity during a conditioned response. *Fed. Proc.*, 1957, **16**, 79. (Abstract)
 31. LEVINE, S. A further study of infantile handling and adult avoidance learning. *J. Pers.*, 1956, **25**, 70-80.
 32. LEVINE, S. Emotionality and aggressive behavior in the mouse as a function of infantile experience. *Amer. Psychologist*, 1957, **12**, 410. (Abstract)
 33. LEVINE, S. Infantile experience and consummatory behavior in adulthood. *J. comp. physiol. Psychol.*, 1957, **50**, 609-612.
 34. LEVINE, S. Infantile experience and resistance to physiological stress. *Science*, 1957, **126**, 405.
 35. LEVINE, S., ALPERT, M., & LEWIS, G. W. Infantile experience and the maturation of the pituitary adrenal axis. *Science*, 1957, **126**, 1347.
 36. LEVINE, S., & OTIS, L. The effects of handling before and after weaning on the resistance of albino rats to later deprivation. *Canad. J. Psychol.*, 1958, **12**, 103-108.
 37. MCCLELLAND, W. J. Differential handling and weight gain in the rat. *Canad. J. Psychol.*, 1956, **10**, 19-22.
 38. MACDONALD, G. E., & TEGHTSOONIAN, R. Weight gain in the albino rat as a function of early visual experience. Paper read at Canadian Psychological Association, Toronto, June 7, 1957.
 39. MACHNE, XENIA, & SEGUNDO, J. P. Unitary responses to afferent volleys in amygdaloid complex. *J. Neurophysiol.*, 1956, **19**, 232-240.
 40. MILLICAN, R. C., & STOHLMAN, E. F. Relative effectiveness of certain drugs against shock produced in mice from tourniquet and burn trauma. *Amer. J. Physiol.*, 1956, **185**, 195-200.
 41. MOGENSON, G. J., McMURRAY, G. A., & JAQUES, L. B. Effects of stress and administration of cortisone on weight gain in gentled rats. *Canad. J. Psychol.*, 1957, **11**, 123-127.
 42. MORGAN, C. T., & STELLAR, E. *Physiological psychology*. (2nd ed.) New York: McGraw-Hill, 1950.
 43. MORGANE, P. J., & KOSMAN, A. J. Alterations in feline behavior following bilateral amygdectomy. *Fed. Proc.*, 1957, **16**, 90. (Abstract)
 44. NELSON, D. H., & SAMUELS, L. T. A method for the determination of 17-hydroxycorticosteroids in blood: 17-hydroxycorticosterone in the peripheral circulation. *J. clin. Endocrinol.*, 1952, **12**, 519-526.
 45. PORTER, R. W. Alterations in electrical activity of the hypothalamus induced by stress stimuli. *Amer. J. Physiol.*, 1952, **169**, 629-637.
 46. PORTER, R. W. Hypothalamic involvement in the pituitary-adrenocortical response to stress stimuli. *Amer. J. Physiol.*, 1953, **172**, 515-519.
 47. PRIEBRAM, K. Concerning the neurophysiological correlates of limbic system stimulation. In D. E. Sheer (Ed.), *Electrical stimulation of the brain*. Houston, Texas: Univ. Houston Press, in press.
 48. RANSON, S. W., & CLARK, S. L. *The anatomy of the nervous system*. (9th ed.) Philadelphia: Saunders, 1953.
 49. REDGATE, E. S., & GELLHORN, E. Relation of anterior hypothalamic excitability to

- cardiovascular reflexes. *Amer. J. Physiol.*, 1955, **183**, 654. (Abstract)
50. RICHTER, C. P. Domestication of the Norway rat and its implications for the problems of stress. In H. G. Wolff, S. G. Wolf, Jr., & C. C. Hare (Eds.), *Life stress and bodily disease*. Baltimore: Williams & Wilkins, 1950. Pp. 3-18.
 51. ROSEN, J. Dominance behavior as a function of early gentling experience in the albino rat. Unpublished master's thesis, Univer. of Toronto, 1957.
 52. ROSS, C. A., & HERCZEG, S. A. Protective effects of ganglionic blocking agents on traumatic shock in the rat. *Proc. Soc. exp. Biol., N. Y.*, 1956, **91**, 196-199.
 53. RUEGAMER, W. R., BERNSTEIN, L., & BENJAMIN, J. D. Growth, food utilization, and thyroid activity in the albino rat as a function of extra handling. *Science*, 1954, **120**, 184-185.
 54. RUEGAMER, W. R., & SILVERMAN, F. R. Influence of gentling on physiology of the rat. *Proc. Soc. exp. Biol., N. Y.*, 1956, **92**, 170-172.
 55. SAYERS, G. The adrenal cortex and homeostasis. *Physiol. Rev.*, 1950, **30**, 241-320.
 56. SCHARRER, E. The general significance of the neurosecretory cell. *Scientia*, 1952, **46**, 177-183.
 57. SCHARRER, E. Neurosecretion and the anterior pituitary in the dog. *Experientia*, 1954, **10**, 264-266.
 58. SCHREINER, L., & KLING, A. Rhinencephalon and behavior. *Amer. J. Physiol.*, 1956, **184**, 486-490.
 59. SCOTT, J. H. Some effects at maturing of gentling, ignoring or shocking rats during infancy. *J. abnorm. soc. Psychol.*, 1955, **51**, 412-414.
 60. SELYE, H. *Stress*. Montreal: Acta, 1950.
 61. SELYE, H., & FORTIER, C. Adaptive reactions to stress. In H. G. Wolff, S. G. Wolf, Jr., & C. C. Hare (Eds.), *Life stress and bodily disease*. Baltimore: Williams & Wilkins, 1950. Pp. 3-18.
 62. SHELLY, CAROLYN, PASAMANICK, B., & LEVINE, S. The effects of infantile experience upon adult behavior in the albino rat. Paper read at American Psychiatric Association, Chicago, May 16, 1957.
 63. SHIMAZU, K., OKADA, M., BAN, T., & KUROTSU, T. Influence of stimulation of the hypothalamic nuclei upon the neurosecretory system in the hypothalamus and the neurohypophysis of rabbit. *Med. J. Osaka Univer.*, 1954, **5**, 701-727.
 64. STRONG, O. S., & ELWYN, A. *Human neuroanatomy*. Baltimore: Williams & Wilkins, 1948.
 65. SYDNOR, KATHERINE L., & SAYERS, G. A technic for determination of adrenocorticotrophin in blood. *Proc. Soc. exp. Biol., N. Y.*, 1952, **79**, 432-436.
 66. THOMPSON, W. R. Paper read in panel discussion, Effects of early experience. Canadian Psychological Association, Toronto, June 7, 1957.
 67. WEININGER, O. Mortality of albino rats under stress as a function of early handling. *Canad. J. Psychol.*, 1953, **7**, 111-114.
 68. WEININGER, O. Physiological damage under emotional stress as a function of early experience. *Science*, 1954, **119**, 285-286.
 69. WEININGER, O. The effects of early experience on behavior and growth characteristics. *J. comp. physiol. Psychol.*, 1956, **49**, 1-9.
 70. WEININGER, O., MCCLELLAND, W. J., & ARIMA, R. K. Gentling and weight gain in the albino rat. *Canad. J. Psychol.*, 1954, **8**, 147-151.
 71. WEISKRANTZ, L. Behavioral changes associated with ablation of the amygdaloid complex in monkeys. *J. comp. physiol. Psychol.*, 1956, **49**, 381-391.
 72. WOODS, J. W. Differences in adrenal response to adverse conditions in wild and domesticated Norway rats. *Fed. Proc.*, 1953, **12**, 159. (Abstract)

(Received February 4, 1958)

FUNDAMENTAL DIMENSIONAL PROPERTIES OF THE OPERANT¹

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At the outset of any inquiry into behavior, and prior to the introduction of experimental variation, the investigator prepares to perform two classes of operations. In the first place he locates and circumscribes, in space and time, the subject matter with which he is concerned. Secondly, he designates some specific dimension or dimensions along which quantities of his subject matter can be established. It is the purpose of this paper to describe the simplest set of operations that can be used to establish quantities of behavioral subject matter. However, the acceptability and meaning of any description of a fundamental dimensional set will depend, in part, upon what unit of subject matter is chosen for purposes of quantification. To put it simply, *how* one sets about to measure largely depends upon *what* one sets about to measure.

Attempts at systematic designation of behavior dimensions have been hampered by the confusion about just what events enter into the subject matter which the dimensions are to describe. If we look at what measures behaviorists actually use we see an indefinite number of dimensions. Latency, resistance to extinction, habit strength, amplitude, speed, accuracy, threshold, frequency, reaction-potential, aggression, mels, VTE, rate, and anxiety are only the beginning of the list of the most common "dimensions." If we reflect upon these many different measurements we should find that many of them are, for the sake of measurement, reducible to others. For an example,

resistance to extinction, as a "measure" of reflex strength, is established by measuring frequency, amplitude, or latency after reinforcement has been withdrawn. Resistance to extinction is a phrase we use to describe changes in behavior, measured along some dimension, as a function of a certain kind of experimental variation. In short, resistance to extinction designates a functional relationship. Threshold is another case in point. We do not "measure" threshold, but we measure the frequency with which one says, "yes, I see the light," and we describe how the frequency of this behavior is functionally related to quantitative changes in the experimental variable. We then, sometimes, worry our heads over the fact that this functional relationship is nonlinear and does not interest the two zeroes simultaneously. Seldom, in science, are functional relations between two variables linear, and seldom does the relationship intersect the variables at the point of origin. This is no ground for leading us to conclude that basic psychological measurement dimensions do not have the characteristics of ratio scales.

It is the first business of science to describe functional relations. It carries out this business by introducing experimental variation and measuring the concurrent changes in the subject matter. In order to describe these functional relations, some operations of measuring the subject matter necessarily are established *prior* to the introduction of the experimental variations. In any science, functional relationships exist in indefi-

¹ This research was supported, in part, by the Graduate School, University of Georgia.

nite numbers, while measurement dimensions do not.

The operations of measuring behavior and the operations of establishing functional relations must be independent of each other, lest confusion is to arise. The procedures we use to describe behavior are established at the beginning of inquiry and cannot change unless the inquiry itself is redirected. Descriptive procedures are required to be independent of experimental variation, since these procedures are utilized for describing the effects of the experimental conditions. This is not to say that the *quantities* obtained from descriptive procedures must be independent of the experimental variables; rather, the *procedures* will be independent if the dependence of the quantities is to have meaning. A set of measurement dimensions is established for the purpose of describing functional relationships and will, therefore, not include the very functional relationships to be described.

A second difficulty in dimensional analysis lies in the confusion about what is meant by dimensional independence, or unidimensionality. The precise distinction that it is necessary to make at the outset is that each member of the fundamental set represent an independent procedure. This does not mean that the outcomes of measurement along different dimensions will be uncorrelated. It simply means that there is nothing in the procedures themselves that necessitates correlation. When we ask how many dimensions a house possesses, we do not first obtain data on the house; rather we first decide along what dimensions houses are measurable, regardless of the particular quantities obtained in the measurement of any one house. We say that a house is multi-dimensional simply because there is more than one procedure by which we can measure it. A dimension implies an operation for obtaining data inde-

pendent of the particular quantities obtained on one or more occasions. However, in the study of behavior, dimensional analysis is frequently thought to be a procedure for ordering data already obtained independently of the methods used to obtain it. Thus, we let the unidimensional characteristics of a set of data hinge upon the data's being uncorrelated with some other set of data. It is not surprising that "unidimensionality" has been hard to come by. It is a well known fact that the physical dimensions of mass and extension are frequently correlated in nature. Nevertheless, we think of them as independent dimensions because they are operations of measurement that do not necessitate correlation. They are, as I shall say, *logically* independent, although not necessarily *empirically* independent.

The analysis that follows does not, then, refer to operations for establishing quantitative functional relationships, nor of operations for establishing the degree of empirical correlation between a set of measures already obtained. It refers, instead, to the simplest set of operations for obtaining measurements of the behavior of organisms. It seems clear that those who investigate behavior typically investigate that class of behaviors that Skinner has called operant (8, p. 65). Skinner's well known definition is suitable for the purposes of this paper. The dimensions set forth here will, I think, be relevant to the class of respondents as well, although I am not fully satisfied that this is so.

Once the behavior is specified as an operant, it still has no empirically determined quantity. A definition of the operant denotes operations that yield its qualitative characteristics and establish it as subject matter. There remains the problem of describing the simplest possible operations that can be performed in the determination of empirical quantity. These operations, once they are

specified, I shall call primary dimensions. Primary dimensions will be those operations of measuring that yield quantities that are of an irreducible simplicity. These quantities will also possess the characteristics of ratio scales.

Now these quantities will all be derived by the physical operations of the measurement of time and space, and can be reduced to these operations; but in being so reduced, their behavioral convenience will be lost. They are irreducible only in the sense that further reduction takes the quantities out of the realms of behavioral discourse. So long as we regard the phenomena in the realms of psychological investigation as physical and extensible, as I shall do here, measurements in time and space are called for. As Bridgman (1) has said, dimensions are matters of convenience so long as they are compatible with the facts and possess the characteristics of ratio scales. Time measurements are themselves extensible, and can be measured by the distance a hand travels over a clock face. This does not remove the fact of the convenience of time as a primary dimension in physics.

The dimensional operations I shall specify here are not given as the absolute and "real" character of behavioral data, but are offered as convenient methods for assigning quantities to behavioral phenomena. The final test of their fundamental properties will be in the consequences that accrue from their use in the laboratory. As I shall describe them, they will be designations of certain operations that can be carried out by the investigator, who has established an operant as his subject matter. They will not refer to specific characteristics of behavioral data, but to means by which these characteristics can be described. It should also be obvious that, if these dimensions can be designated as more fundamental than other procedures of measurement, these other procedures

must be reducible to the dimensions described here.

THE DIMENSIONS

The task of outlining the nature of the dimensions that I consider primary may be facilitated by imagining a simple laboratory situation. A human subject is brought into a room and seated before a panel on which is mounted a plunger that can be pushed against a spring resistance. The plunger is so built that, if it is released at all, it will return to its normal or starting position before it can be pushed again. The plunger is mounted on a swivel so that the direction of the push is variable. A cumulative recorder is attached to the plunger so that any movement is recorded. The subject is instructed to push the plunger as often as he likes, and told he may earn money for his work. He is told to begin when the experimenter turns on a light and that he can stop working whenever he likes. The fixed reference space, in this case, is the plunger, and the operant is plunger pushing.

Temporal Dimensions

Latency. The first elementary measurement operation is that of recording the time between the opportunity for an operant and its initial occurrence. The measure is stated in time units, and can be easily obtained from a cumulative recorder or a timer that goes into operation when the plunger is made available and cuts off when the plunger is first pushed.

Tempo. Once an operant occurs it is frequently repeated, and there is a unitary quality about the sequence of operants as they are being repeated. For example, the child who pedals a bicycle evidences no break between pedaling through one revolution and another, and one operant leads smoothly into another. The rate of this continuously ongoing

operant is tempo. In the plunger-pushing example, tempo can be easily measured as the slope of the cumulative curve during those times in which *S* is working continuously. This measurement is made quite easily where these slopes are linear, which is more often the case than not. A decision must be made about what is to be considered continuous or discontinuous; however, this decision is taken care of in the consideration of the next dimension.

The measure of tempo as the rate at which the operant is being emitted, exclusive of any breaks in its emission, is dependent upon the duration of the individual operants in the run. The tempo of a single operant is its duration. In order to distinguish between the duration of the operant and the duration of the time in which the organism is emitting operants, I shall borrow a term from physics. We may refer to the duration of a single instance of an operant as the *period* of that operant.

Perseveration. The third temporal dimension can be seen where we regard the amount of time the organism spends, after latency, at the prescribed task. If we regard the total time from the occurrence of the first operant to the completion of the last, the *proportion* of this time in which operants are actually occurring is perseveration. Examples of this type of variation are numerous. For example, after some continuous plunger pushing or bicycle pedaling, a person will pause and engage himself in some other task before continuing with the original one. Just how long the pause must be before we decide that the task has been discontinued is an arbitrary matter. This question creates very little difficulty in normal laboratory situations, however. In the plunger example, perseveration is measured by a timer that is in continuous operation while *S* is pushing the plunger. If *S* pauses for as much as five seconds, a

delay relay shuts off the timer until *S* resumes work. Perseveration can also be calculated directly from cumulative tracings. The five-second pause as a criterion for deciding that *S* has quit the task is arbitrary, but is derived, in this case, from observations of how long it takes the slowest *S* to complete the cycle of one plunger push.

Duration. The fourth temporal dimension is the total time from the initiation of the first operant to the completion of the last, independent of the amount of work done within this interval. The designation of the completion of a sequence of operants will vary from investigation to investigation. It may be in the form of completing a task such as filling up a ditch, reaching some criterion of satiation, or returning to operant-level of output. In the plunger example, it is simply the time from the end of latency until *S* pushes a switch, thus indicating his desire to quit.

Figure 1 is a schematic presentation

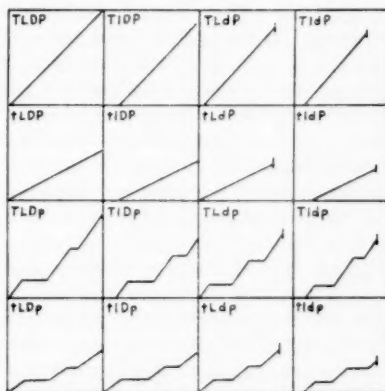


FIG. 1. A schematic presentation of variations along four temporal dimensions, illustrating the independence of each. The figures are cumulative recordings. Upper case letters represent: fast tempo (T), short latency (L), long duration (D), and high perseveration (P). Lower case letters represent: slow tempo (t), long latency (l), short duration (d), and low perseveration (p).

of hypothetical results from the plunger-pushing experiment. The figure represents several cumulative tracings, the ordinate being a frequency scale and the abscissa a time scale. As can be seen readily, the total work output may vary as a function of each of these dimensions, and the variation in each dimension is logically independent of variations in any other dimension.

Output

Total output data will be a function of each of these four temporal dimensions. The dimensions combine in the following manner to yield a total output quantity (where it can be assumed that tempo is linear):

$$R = KTPD - L,$$

where R is output in frequency; P is perseveration in proportion of total time less latency; D is duration in seconds; L is latency in seconds; T is the slope of the cumulative recording where the animal is at work; and K is a correction for different units of measurement of frequency on the ordinate and seconds on the abscissa. KT is the number of operants occurring in a second during the times in which the animal is actually working. It should be noted that the spatial dimensions also enter into the determination of total output. Here we have considered the case in which these dimensions are held at some fixed value.

In the special case in which the task is of some fixed length, duration naturally will be determined by perseveration and tempo. In this event, time rather than output measures are commonly used. It can be seen that time is a complex measure, determined by the operation of several dimensions.

Spatial Dimensions

A most obvious and common measure of an operant in space is what has been called amplitude or magnitude. How-

ever, the behavioral concept of amplitude may involve two independently measurable characteristics. For this reason I have chosen two names that, while not unusual, are seldom used in behavioral description.

Intension. The operation that is usually indicated by the word "amplitude" is the measurement of the amount of movement that takes place in some specified part of the organism's topography. This measure of operant amplitude I shall call intension, and it is taken when we regard the amount of movement that some specified part of the animal makes with reference to some other space. In the plunger experiment, the measure is simply made by recording the distance through which the plunger moves from the normal position. Intension must not be confused with the next measure of amplitude.

Extension. Independently of the amount of movement of a specified part of the animal, we may measure the amount of the animal involved in the movement. A simple illustration of the distinction between intension and extension is made by imagining a hand laid palm down on a table surface. We may regard the distance through which a finger (or fingers) may be lifted (intension), or we may regard, independently of the distance through which they are moved, whether one or more fingers are lifted (extension). Russell et al. (5) have made a somewhat similar distinction in an analysis of quantitative procedures. I believe that what I call extension is similar to what they have called "quantal intensity." A precise terminological comparison is hardly feasible, since our methods of analysis differ rather sharply. The measurement of extension is difficult to make in many behavioral situations, including the plunger apparatus. We may determine whether the plunger is pushed with one hand or two, or we may make

arbitrary divisions of the topography and measure the involvement of these divisions by muscle action-potentials.

Direction. The third spatial dimension is so well known as to require little elaboration. This dimension is established by the measurement of the direction of movement with respect to some fixed reference space. The operation by which this measure is taken from the swivel-mounted plunger should be obvious. Accuracy, error, and "preference" are typical expressions of the direction dimension.

TWO EXPERIMENTAL EXAMPLES OF THE ANALYSIS

Such an analysis as the one offered here should have empirical consequences to establish its worth to an empirical science. I will briefly describe a few representative results from two investigations that may serve to argue for the more widespread usage of this analysis as well as to indicate the empirical properties of several of these dimensions.²

The first experiments represented procedures similar to those of Guttman (3). Although no attempt was made to replicate Guttman's work, our data are not very different from his findings. Essentially we allowed fully trained rats to receive .05 ml. of a sucrose solution from a tube upon pressing a bar (Guttman used .005 ml.). One of the major experimental variables was the concentration of sucrose in solution (4, 8, 16, and 32%). An important feature of the apparatus was the fact that the animal received a drop directly into his mouth as he pressed the bar, and he never had to move away from the bar. In many Skinner boxes, the animal moves away from the bar and the operants cannot

occur with the continuity required for the measurement of tempo.

From rather fast-moving cumulative recorders, the temporal dimensions listed above were measured. Figure 2 summarizes and schematizes the findings. Each of the temporal dimensions is plotted as a separate function of the sucrose concentrations. Figure 2E represents the total output as a function of sucrose concentration, and the general form of this curve is similar to that found by Guttman. However, we can see that the total output is analyzable into four contributing dimensions, each being a different function of the experimental variable.

Tempo. This is an invariant, remaining the same throughout the range of concentrations (Fig. 2C). It was also discovered that tempo did not change as the animal approached satiation; a fact that is supported by earlier findings in this laboratory, as well as by the data of Skinner (7), Young (9), and Sidman and Stebbins (6). This invariant character of tempo seems to be true for both PR and continuous schedules. The generalization is that, when the animal works at a task, he works at a tempo that is characteristic of him and that is unaffected by the nature of his reinforcers or the extent of his deprivation.

Another characteristic of tempo seems to be its moment-to-moment invariance. In our laboratories we investigated the consistency of tempo from day to day and from time to time within a day's run. Reliability coefficients, represented by linear r s, are in the neighborhood of .98.³ There is great variability in tempo between animals, and negligible variability within animals from time to time.

³ This work was done with W. T. James of the University of Georgia, and reported to the Southern Society of Philosophy and Psychology, Atlanta, April, 1954. James is primarily responsible for the design of the liquid reinforcing apparatus which was used.

² The data of the first of these experiments were obtained in collaboration with Lloyd Hitchcock, Jr. (4).

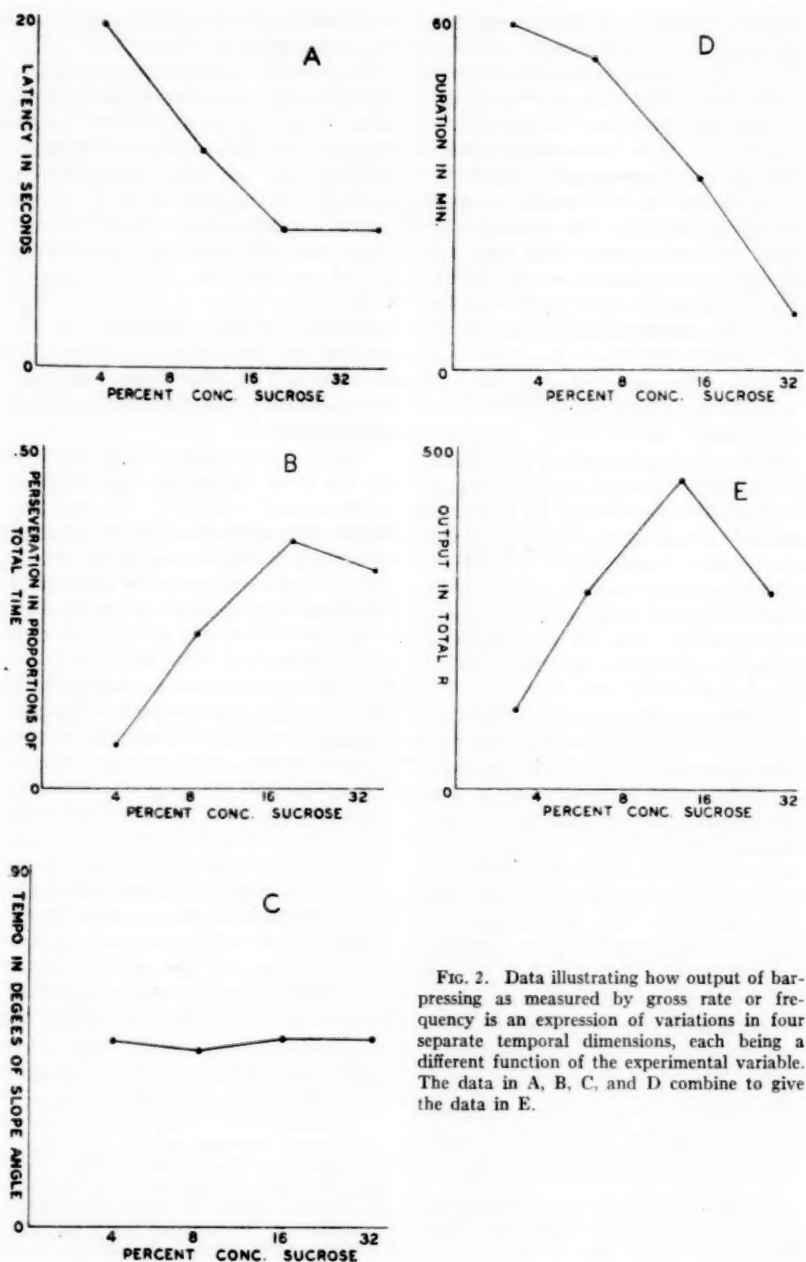


FIG. 2. Data illustrating how output of bar-pressing as measured by gross rate or frequency is an expression of variations in four separate temporal dimensions, each being a different function of the experimental variable. The data in A, B, C, and D combine to give the data in E.

Latency. In the sucrose investigation, it was discovered that latency is a negatively accelerated, decreasing, monotonic function of reinforcing concentrations (Fig. 2A). This is a finding supported by too many investigations to need mentioning. The well known variability in latency measures was also observed here.

Perseveration. This was found to be an increasing, negatively accelerating, monotonic function of sucrose concentration (Fig. 2B). Actually, variations in perseveration determine the greater part of the variations in total output, and seem to be the most sensitive and reliable indices of performance variability. The reliability of these measures is quite high. As the animal approaches satiation, he perseveres less and less at the task, and this fact probably determines the negative acceleration of most work curves.

Duration. In the sucrose reinforcement experiment the animals were allowed to work a fixed time, so that all animals did not reach a reasonable criterion of satiation. However, estimates from these data combined with other data taken in our laboratory suggest the following conclusions: Duration is a positively accelerating, decreasing, monotonic function of sucrose concentration. The greater the concentration of sucrose, the earlier does the animal become satiated (Fig. 2D). However, as deprivation is increased, duration becomes greater—a fact that should be disturbing to anyone who would point to the similarity of the effects of drive and reinforcement on the form of total output curves.

A brief summary of data from the human plunger-pushing apparatus, described above, may serve to indicate some of the generality of these procedures and their outcome. The human subject is requested to push the plunger to work for points. He receives one point after the first 200 plunger pushes,

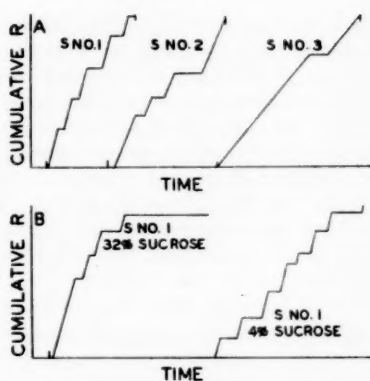


FIG. 3. Sample cumulative tracings illustrating the constancy of tempo and the way in which total output is determined by variation in different dimensions. A represents three human Ss pushing a plunger to earn grade points. B represents one rat working for two different concentrations of sucrose.

and then no more. He is allowed to work as long as he wishes. Figure 3A represents the cumulative recordings of this work for three different Ss. Figure 3B represents the curves of a typical rat working for different amounts of sucrose. As can be seen, tempo is invariant in all Ss, and tempo, perseveration, and duration all serve to determine the final output.⁴

SOME IMPLICATIONS OF THIS ANALYSIS

Consideration of Statistics

It is commonly believed that statistical analysis is a necessity in the behavior laboratory. A few investigators, such as Skinner, have presented persuasive arguments to the contrary. In spite of these arguments, however, data are

⁴ Skinner (7) once demonstrated the use of a water operant. The curves he presents are similar to those here, and the constancy of tempo can be readily seen, although his technique was slightly different. He rejected the water operant partly on the grounds of the irregularity of the curves obtained. The irregularity indicated breaks in the work periods.

often so variable as to appear understandable only by way of statistical analysis. It is usually assumed that this great variability is produced by uncontrolled and unknown "random" conditions in the laboratory, and that statistical analysis allows us to estimate and account for these "random" effects. I shall not deny the grounds for the validity of this assumption; however, I believe that this is a much overworn argument. I contend that the compound nature of much of our measurement is an important source of data variability. In the experiment described above, we obtained curves shaped like those of Guttman (3). However, with twelve animals a statistical analysis yielded no significant *F*s. Guttman had to use several dozen animals to obtain *F*s that were only moderately significant. When we analyzed the perseveration scores separately, there was no question about statistical significance. Such significance was found using the analysis of variance, but it was quite obvious without the use of statistics. This was more remarkably the case with tempo scores.

If, as I have contended, an animal's tempo is invariant under a wide range of experimental conditions, it can only serve to attenuate differences in total output scores produced by experimental variation. If an animal spends most of his time at a task, and if he works at a tempo unaffected by the experimental conditions, the tempo will be the greatest determinant of the total output measure. The output score will then consist of a component reflecting not experimental variations but great interindividual differences. In the language of statistics this could only serve to increase the "error term."

Misleading Characteristics of Total Output Measures

To exemplify the greater need for the type of analysis I have proposed, an

experiment by Fink and Patton (2) will be scrutinized. These investigators studied the effects of changes in the population of stimuli on the decrement of conditioned drinking behavior. Rats were taught for five days to drink from a burette. On the sixth day various kinds and degrees of stimulus changes were made in the drinking box, and measurement was made of the water intake during the first two minutes following the stimulus changes. It was concluded that "the greater the number of stimulus components changed, the greater the response decrement" (2, p. 27). The findings of this experiment are interpreted by the authors as giving support to the S-R contiguity theory. The authors note that their measure could reflect either rate or amplitude, and point that that the Estes-Burke behavior models have not yet made differential formulations for rate and amplitude. However, it is important to notice that rate is not, itself, a simple measure. Should we assume that their data reflect no changes in amplitude, the interpretation would remain quite difficult. The decrement in the drinking reflex could be due to changes in latency. Perhaps the animals were "startled." If the decrement did not reflect latency changes, but decreasing tempo, the meaning of the findings should be considerably changed. Similarly, changes in perseveration or duration would be reflected in the total output during the two minutes and would deserve different interpretations. If "amplitude" changes account for the decrement, it must be shown whether these changes were in intension or extension. Precision in mathematical and verbal formulations of theory must always follow precision in the measurements on which they are based. Otherwise, the precision is specious indeed.

The Fink and Patton experiment is only illustrative. The amount of pos-

sibly misleading output data appearing in behavioral journals would require a separate review. Experimentation on work may serve as another general example. Data from work experiments are notoriously variable and the uncontradicted conclusions about this field are few. Output measures are favored, and they reflect each of the proposed dimensions as they combine to yield a total score. Precise answers about, say, optimum rest periods will await the study of the effects of these rest periods at different tempos, perseverations, durations, intensions, and extensions. Similarly, questions about optimum tempo will have answers dependent upon the relationship of tempo to perseveration, duration, extension, etc.

A Word About "Mental Tests"

Intelligence test scores are compounds, not only of measurements of different operants, but of various dimensions of several of the separate operants. Thus, the Wechsler-Bellevue Digit Symbol test score will vary as the examinee perseveres during the task, as a function of his tempo, as a function of latency, as a function of direction (accuracy) and perhaps as a function of intension and extension when he uses the pencil. Only duration is fixed. Two identical Digit Symbol scores may have entirely different meanings as a result. Whatever questions concerning the construction of ratio-scales of measurement exist, one thing seems clear: If scores are constructed from the undifferentiated combinations of seven different ratio scales, no one may reasonably hope that the result will be a ratio scale, regardless of what further operations are performed upon these scores. No two IQs are ever likely to mean the same things when such measurements are uncritically combined. Even when the tests seem to meet the criteria of ratio scales, as the Stanford version of

the Binet-Simon so nearly does,⁵ the comparisons are still meaningless in any but the crudest sense. This is not because the tasks required are different, since they can all be considered as belonging to the same task class; it is because the tasks are measured differently or by compound procedures. Equal things, measured differently, are not equal.

SUMMARY

Seven dimensions of an operant were described as fundamental in the sense that other operations of behavior measurement are reducible to these seven and the seven are not further reducible within the realms of behavioral discourse. Certain operating assumptions were outlined as a basis for the analysis.

The seven dimensions are (a) *latency*, defined as the time between opportunity for an operant and its initial occurrence; (b) *tempo*, defined as the rate of emission of a continuously ongoing operant, or the period of a single operant; (c) *perseveration*, defined as the proportion of the time from the occurrence of the first operant to the completion of the last in which operants are actually occurring; (d) *duration*, the total time from the initiation of the first operant to the completion of the last; (e) *intension*, the amount of movement that takes place in an operant involving some specified part of the organism's topography; (f) *extension*, the degree of the topography involved in an operant; and (g) *direction*, i.e., of the organism's movement with respect to some fixed reference point.

Empirical characteristics of several of these dimensions were described. An experiment, partly modeled after Gutt-

⁵ E. E. Cureton, personal communication. Cureton has shown how this test has nearly equal SDs from the ages of 3 to 13, and how the scale, if extrapolated, places zero M.A. at birth.

man's (3) work on sucrose as a reinforcer, was performed to indicate how each of several dimensions may be a different function of an experimental variable, and combine to give a peculiar output function. Implications of the analysis were discussed, with particular reference to the pitfalls of uncritically combining the dimensions.

REFERENCES

1. BRIDGMAN, P. W. *Dimensional analysis*. New Haven: Yale Univer. Press, 1931.
2. FINK, J. B., & PATTON, R. M. Decrement of a learned drinking response accompanying changes in several stimulus characteristics. *J. comp. physiol. Psychol.*, 1953, **46**, 23-27.
3. GUTTMAN, N. Operant conditioning, extinction, and periodic reinforcement in relation to concentration of sucrose used as reinforcing agent. *J. exp. Psychol.*, 1953, **46**, 213-223.
4. HITCHCOCK, L. The performance of an operant in relation to the concentration of sucrose used as reinforcing agent. Unpublished M.S. thesis, Univer. of Georgia, 1956.
5. RUSSELL, W. M. S., MEAD, A. P., & HAYES, J. S. A basis for the quantitative study of the structure of behavior. *Behaviour*, 1954, **6**, 153-205.
6. SIDMAN, M., & STEBBINS, W. C. Satiation effects under fixed schedules of reinforcement. *J. comp. physiol. Psychol.*, 1954, **47**, 114-116.
7. SKINNER, B. F. Thirst as an arbitrary drive. *J. gen. Psychol.*, 1935, **12**, 40-65.
8. SKINNER, B. F. *Science and human behavior*. New York: Macmillan, 1953.
9. YOUNG, P. T., & RICHEY, H. W. Diurnal drinking patterns in the rat. *J. comp. physiol. Psychol.*, 1952, **45**, 80-89.

(Received October 14, 1957)

THE DEFINITION AND ANALYSIS OF PERCEPTUAL LEARNING¹

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In recent years considerable attention has been given to the question of how, and to what extent, learning may influence perception. Most frequently this question has been answered by recourse to intervening constructs such as organizations, hypotheses, assumptions, schemata, etc., as mediators of changes in the perception of a given stimulus as a function of experience. This approach has been aptly termed by Gibson and Gibson (16) the "enrichment theory," since it takes "for granted a discrepancy between the sensory input and the finished percept and . . . aim[s] to explain the difference. . . . The development of perception must then necessarily be one of supplementing or interpreting or organizing" (16, p. 33). Against this view the aforementioned authors oppose an interpretation of their own, emphasizing progressive differentiation of the stimulus as the essential element in perceptual learning. According to this "specificity theory,"

. . . we learn to perceive in this sense: that percepts change over time by progressive elaboration of qualities, features, and dimensions of variation. . . . Perceptual learning, then, consists of responding to variables of physical stimulation not previously responded to (16, p. 34).

This thesis, as elaborated in their paper, did not long remain unchallenged. In an extended critique by Postman (30), it was argued that the specificity theory begged the question of learning,

i.e., of *how* the increased differentiation of the stimuli was mediated so as to result in differential responses. In this connection Postman maintains that "the changes in response are part and parcel of the problem of perceptual learning. The need to account for changes in response inevitably endows the problem of perceptual learning with an associative component" (30, p. 442). But even taking the specificity hypothesis as a descriptive generalization of the *results* of perceptual learning, Postman argues that it is of rather limited generality: for instance, it cannot account for the perception of signs and symbols.

It seems impossible to evaluate these criticisms without some more clearly specified criterion of perceptual learning than is provided by either of the parties to this dispute. Thus the question whether perceptual learning is a matter of attaching responses to stimuli depends on the nature of the S-R relationship that is to be learned. In a typical paired-associate learning task—e.g., learning to identify a set of unfamiliar faces by name—it is evident that the association of the correct response plays a major part in the learning process. This is not to deny, of course, the possible role of perceptual processes, but it is in the very nature of the case that these two aspects cannot be disentangled. In a situation, on the other hand, in which the subject makes judgments of "same" or "different," the attaching of responses need hardly be considered, as will be shown below. As for Postman's second criticism, one is inclined to agree that in the learning of meaningful signs and symbols a stress on the

¹ This paper was written in part during the author's tenure of a NSF postdoctoral fellowship at the University of Geneva. The author is indebted to Jan Smedslund for a critical reading of the manuscript.

differentiation of characteristics of the stimulus is generally beside the point. The question, however, is whether a theory of perceptual learning need, or in fact should, encompass this kind of task at all. The answer again hinges on the criterion for classifying a learning task as perceptual.

It is thus the intent of this paper to propose such a criterion, which will help us in evaluating the arguments brought forth in this controversy and, more important, enable us to undertake a more systematic analysis of the nature of perceptual learning in a variety of specific situations.

A CRITERION FOR PERCEPTUAL LEARNING

In formulating this criterion, our main aim will be to exclude from consideration situations which necessarily involve the formation of new stimulus-response associations, since in such situations the responses cannot be used to index changes on the perceptual side. Accordingly, the basic ingredient for our definition of perceptual learning will be the use of responses which have previously been established to a class of stimuli or to a stimulus relationship; if the stimuli which are to be discriminated represent instances of this class or relationship, the response to it *need not* then be newly associated. Perceptual learning might thus be regarded as the development of a transfer of a previously learned set of responses to a new set of stimuli, the possibility for this transfer inhering in the physical characteristics of the stimuli. In other words, each stimulus to be discriminated must be classifiable, *qua* physical stimulus, into the class of stimuli to which the corresponding differential response is preattached, and similarly for stimulus relationships.

By this definition, then, paired-associate learning and similar tasks are left

outside the province of perceptual learning situations. We will return later to the problem of assessing the role that perceptual learning may play in such tasks. For the moment it may be noted that this restriction likewise excludes the learning of signs and symbols, so that Postman's second criticism, referred to above, is avoided.

In a more positive sense, the criterion proposed above has the advantage of confronting us with a clear-cut issue concerning the nature of perceptual learning. An associational basis is no longer forced upon us; instead, two alternatives now emerge. On the one hand, the transfer which the situation provides for may take place in fact. In this case perceptual learning may be interpreted in terms of progressively more effective abstraction of those features of stimulation which represent the basis for the transfer of the correct response. On the other hand, this response may be established through association, direct or mediated, with *any* differentiating aspect of the respective stimuli, through differential reinforcement.

Although the first alternative leaves open the precise nature of the mechanism by which the transfer comes to be established, it entails a variety of experimental consequences serving to distinguish it from the second alternative. One of these may be stated at the outset: if associational mechanisms are involved, learning should take place only as a function of external reinforcement—i.e., the subject's response must have some consequence informing him of its correctness or aptness. But such reinforcement need not intervene in learning which conforms to the first alternative, since this is essentially a generalization phenomenon. Other implications of this distinction will be brought out below, in the discussion of specific experimental problems.

This brings us back to the interpretation of perceptual learning offered by Gibson and Gibson (16). Their specificity theory, based on stimulus differentiation instead of mediational processes, may be classed with our first alternative (perceptual learning as transfer, through abstraction of relevant aspects of the stimulus), while the "enrichment theory" to which they oppose themselves conforms to the second (perceptual learning through association).

The above two formulations of perceptual learning may, then, be said to differ with respect to their conceptions of what is learned. As the Gibsons note in their reply to Postman (17), it is precisely this question which their specificity hypothesis is meant to clarify. In order to explore this question more specifically, however, it will be essential to examine a variety of particular situations. Indeed, we shall see presently that the answer to this what-is-learned question depends to a considerable extent on the particular discriminative judgment made by the subject.

ANALYSIS OF PERCEPTUAL LEARNING SITUATIONS

It is evident that, by the very nature of the criterion adopted, perceptual learning must be considered relative to a particular set of available responses. For the purposes of the following survey, therefore, the various kinds of situations will be differentiated according to the type of discriminative response demanded of the subject.

On this basis three broad categories of tasks may be distinguished: discrimination of identity vs. difference, quantitative discriminations, and qualitative discriminations. Each of these last two can be further subdivided into a general and a specific type. Thus we have, with respect to the quantitative category, judgments of relationship and ordered magnitude judgments; and, with respect

to the qualitative category, categorization and identification.

Type I: Identity vs. Difference

This type represents undoubtedly the most basic kind of discrimination; it is applicable to any stimulus material whatever, simple or complex, familiar or unfamiliar. For this reason it lends itself ideally to the study of perceptual learning; yet the Gibsons, in their exploratory experiment illustrating their specificity hypothesis (16), seem to be the only ones to have realized the possibilities of this form of discriminative judgment for the investigation of perceptual learning.

The study of this type of differentiation requires only the prior acquisition of the differential responses to identity and difference in a group of stimuli.² These responses are, furthermore, completely nonspecific with respect to the aspects of the stimuli to which they refer: any differentiating characteristic among the set of stimuli constitutes a potential basis for the response. The abstraction of such a characteristic is therefore a necessary and sufficient condition for the transfer of the response, so that intervening associative processes may be considered beside the point. For this type of learning, in fact, reinforcement may be altogether unnecessary, as was the case in the experiment by the Gibsons just cited. In a sense their hypothesis is thus true here by definition: in this situation perceptual learning necessarily consists in making finer discriminations among the sources of variation present in the stimuli.

²For nonverbal subjects, these responses could be established through preliminary training by the matching-from-sample or oddity techniques, directed at the formation of learning sets. The success which monkeys (cf. 19) and even birds (24, 27) have had in solving problems of this type argues itself for the rudimentary nature of this form of discrimination.

It is possible, however, to interpret this hypothesis in a more specific sense, i.e., as postulating discrimination along continuous dimensions of variation, as opposed to the isolation of discrete cues. Thus, in learning to differentiate a set of faces, does the observer pick out individual details (a crooked nose, a double chin, etc.), or does he discriminate dimensional differences (width of mouth, ratio of distance between eyes to that between forehead and chin, etc.)? The importance of this question lies in the fact that the generalizability of the learning is dependent on which of these two forms of learning occurs. If the discrimination is dimensional, the learning should transfer to a new sample of faces, whereas little or no transfer is to be expected from the isolation of specific cues.

It appears likely that the complexity of the stimuli will determine in large measure the extent to which dimensional aspects of the stimuli will be discriminated. Thus the nonsense shapes generated according to information-theory principles (6) suggest themselves as promising materials for the study of this question.

Type II: Judgments of Quantitative Relationships

Here we find represented the work on the reduction of simple differential thresholds through practice, which accounts for a considerable portion of the perceptual learning literature (cf. the review by E. Gibson [10]). But this category includes in addition a variety of more complex problems, such as the perceptual constancies, illusions, etc. The essential element is the presentation of stimuli varying on some physical dimension, in accordance with one of the classical psychophysical methods (other than that of single stimuli), or some other procedure involving comparative judgment. An additional re-

quirement, following from our definition of perceptual learning, is that the subjects have previously applied the response categories to the dimension which is manipulated. Although this stipulation rarely presents a problem with verbal subjects, it does create a judgment situation somewhat different from that in Type I.

In order to illustrate the characteristic features of this type of judgment, let us consider a subject who has been asked to judge the relative distance of two objects. The first point to be noted is that, in contrast to Type I situations, the differential responses refer now specifically to a particular physical dimension. But in the subject's prior experience with this dimension, a complex of sensory information, both dimensional (e.g., linear perspective, gradients of texture) and nondimensional (e.g., interposition), has necessarily been correlated with the physical dimension of distance. One might say, in fact, that the concept of distance, connotatively considered, is linked to this complex of stimulation.

Accordingly, in order to show improvement due to practice in his distance judgments, our adult subject need only scan the stimulus field more effectively, so as to abstract some of this correlated information. This view would seem to represent the position taken by the Gibsons on the problem of perceptual learning. Essentially descriptive though it remains, such an interpretation may yet be strengthened against the charge of circularity, to the extent that the experimenter succeeds in specifying the particular variables of stimulation which are being differentiated in this situation. The exploration of such variables is of course the aim of the "global psychophysics" advocated by Gibson (15).

Are other explanations for the learning observed in this situation possible?

If this learning took place in the absence of external reinforcement, it is difficult to conceive what they might be, except perhaps for general warm-up or learning-to-learn effects. The associational alternative, in any case, would scarcely be applicable, since the formation of S-R associations would seem to presuppose some reinforcing agent.

If, on the other hand, reinforcement is given, the associational hypothesis clearly becomes relevant. Learning may consist, then, in a direct association of the correct responses with some arbitrary differentiating cues in the stimulus objects. One readily testable implication of this formulation may be pointed out: it should be possible to establish increasingly nonveridical judgments as easily as increasingly veridical ones, simply by reversing the pattern of reinforcements. Any difference in favor of veridical learning would support the first explanation, in terms of direct transfer through improved differentiation of relevant stimulus variables.

Judgments of distance represent, of course, only one kind of task, and a relatively complex one, among the many different problems included under Type II. It is relevant to ask, therefore, whether the above discussion is applicable generally to other problems of this type. The answer depends on the complexity of the stimulus correlates of the physical dimension selected, in the context of the situation in which the judgment is to be made. Where these correlates are as univariate, as dimensionally simple as the physical dimension itself (e.g., the loudness of pure tones in a noiseless chamber, the brightness of flat surfaces under constant illumination), there appears to be no opportunity for the kind of perceptual learning postulated by the specificity hypothesis. It is perhaps significant, in this connection, that in her extensive review of the perceptual learning literature (10), E.

Gibson cites no instance of improvement in differential thresholds for these intensive dimensions. For most supposedly simple stimulus dimensions, however, the sensory input is of a somewhat higher order of complexity. Particularly is this true in the case of vision, where there are almost always some relationships within the stimulus field that vary concomitantly with the dimension being manipulated. For example, in as relatively bare a stimulus field as that represented by the lines for the horizontal-vertical illusion, variation of the vertical will be accompanied by changes in the (virtual) triangle formed by the vertical, the horizontal, and the line joining the extremities of these two. As for the still more complex, multivariate stimulus fields generally encountered outside the laboratory, it is apparent that the opportunity for the utilization of a variety of correlated information in the proximal stimulus is often considerable—a point which the Gibsons are at pains to emphasize (16).

Type III: Ordered Magnitude Judgments

If, for the relational responses of Type II tasks, we substitute an ordered set of responses serving to identify the magnitude of a corresponding set of points on a physical scale, we arrive at a Type III task, commonly known as scale learning. Two aspects of scale learning may be distinguished: the absolute and the relative. It will become apparent that, when only the second of these aspects is considered, the general nature of the problem resembles in many ways that of a Type II task. But since scale learning has typically been studied in regard to its absolute aspect, let us start by examining it.

Scale learning in the absolute sense consists in the identification of the points on the stimulus scale in terms of their absolute physical magnitude. The

difficulty with this aspect, from the standpoint of the present approach to perceptual learning, is that one can rarely be confident that the differential responses have been learned beforehand. Thus one would first have to establish them through training, as E. Gibson and her associates have done for judgments of distance over the ground (11, 12). It was demonstrated in these studies that this training transfers to subsequent judgments of a new set of points on the same field (11), and even to points on a different terrain (12). This evidence is, however, of equivocal relevance to the question of perceptual learning, since the training consisted in the establishment of the responses, and there was no opportunity for learning during the transfer series itself. Gibson and Bergman (11), as a matter of fact, recognize the ambiguity of their results, for they point out that the transfer they found may reflect chiefly the improved conceptualization of the stimulus scale, through a more accurate notion of the unit of measurement.

An alternative possibility, discussed in part by these authors, appears of greater interest for the study of perceptual learning: the subjects may have become more accurate in their discrimination of the relative ordering of the points, and of the relative distances between them. This brings us to the relative aspect of scale learning, which, although it has not been explicitly studied thus far, leads itself much more readily to an analysis in conformance with our definition of perceptual learning.

For this aspect requires only that the subject have learned a set of responses conforming to the ordinal, interval, or ratio character of the stimulus scale. He would then be in a position to apply this response set to the perceived ordering of the points on the stimulus scale, and, where applicable, to the perceived metric of the scale itself. We

would thus be interested primarily in the regression, linear or otherwise, of the judgments on the objective values, and the problem of perceptual learning would reduce to the approach of this regression to unity.

The above approach rests, of course, on the assumption that the subject's numerical responses reflect the metric character of the perceived magnitudes. Garner, Hake, and Eriksen (9) have questioned the validity of this assumption, pointing out that the subjects may use the numbers, not in terms of their arithmetic relationships, but rather as identifying responses, or perhaps as a set of ordered responses. The former would be true particularly where the subject has a record of prior associative experience in the application of numbers to different magnitudes on a dimension, as in the case of size, where particular values are frequently associated with individual objects.

This is certainly a valid criticism, but it may not be critical in the study of perceptual learning. For the question remains how to account for the *improvement* in a set of judgments in a learning task. It may be, of course, that the opportunity for the association of numbers with individual points on the scale is provided by the learning situation itself. An example of this kind may be seen in an earlier study of distance estimation by Gibson and Smith (13), in which it was observed that the subjects merely learned to associate specific, isolated cues on the terrain corresponding to a given distance with the verbal response which was reinforced for that distance. But it should be possible to spot learning of this direct associative character by means of transfer tests involving other points on the scale, or a change in context, or discrimination on other, correlated dimensions. (In this particular study, the authors were interested in transfer to size-at-a-distance

judgments. Not surprisingly under the circumstances, none was found.) If, in terms of the relative aspect of the scale, such transfer is found, or if learning occurs without benefit of reinforcement, it would seem to justify the inference that the subjects actually did learn to perceive more accurately the relative magnitude of the points on the scale.³ This learning is then presumably attributable to improved discrimination of relevant stimulus variables, so that the problem of the nature of perceptual learning becomes essentially equivalent to that discussed under Type II situations.

Since the discussion so far has centered almost exclusively on the scale of distance over the ground, it is important to note that this scale is not only particularly rich in associated variables of stimulation, as noted in the previous section, but represents a rather special case, insofar as all of the points on the scale are continually exposed to the subject's view. This property clearly contributes to the subject's chances of effectively ordering a set of designated distances. For other visual scales (area, brightness) one might similarly expose the scale in its entirety, but for dimensions on which only one or a few magnitudes at a time can be exposed (pitch, weight), the situation is probably not comparable. At the very least it would be essential with such scales to present anchoring stimuli, identified as such, at frequent intervals during the learning task. Otherwise the subject's response scale might easily "drift," thereby rendering the results quite equivocal with respect to the relative aspect of the scale judgments.

³ It is conceivable, to be sure, that the learning may reflect the gradual abandonment by the subject of preferential response habits, e.g., the tendency to respond in round numbers. This reservation points to the importance in perceptual learning experiments of eliminating differential response strengths in advance; cf. the discussion of Type V tasks below.

Type IV: Categorization

This type of perceptual learning is concerned with the placing of unfamiliar stimuli into a set of nondimensional categories previously learned by the subject. The differential responses are thus the concepts representing the categories; they are preassociated to a class of stimuli sharing certain physical characteristics, which are present in less readily discriminable form in the stimuli to be categorized.

This problem apparently still awaits experimental attack, but one way of approaching it might be as follows: sets of nonsense shapes varying around a prototype—as described in the recent experiment by Attneave (5)—would be selected to establish an artificial set of categories; other shapes constructed so as to be further removed from their respective prototypes would then be presented for categorization. The question here is whether the subjects would, with practice, succeed in making the necessary discriminations along the variables on which the categories could be differentiated, or whether the categorization of the new stimuli would proceed by direct association, through reinforcement. This problem is clearly relevant to the study of concept formation: does a child, in learning to classify a duck as a bird, actually discriminate the features of the stimulus object which relate it to other birds with which the child is already familiar, or does he merely associate the term "bird" with this new stimulus? The evidence on this kind of question is lacking, however. To be sure, a number of concept-formation studies have manipulated variables relating to the stimulus (e.g., 7, 26), but the interest has been limited to the effect of such variables on the speed of acquisition of the concepts, rather than on the kind of discrimination of the stimuli achieved.

Type V: Identification

The identification of stimuli by responses specific to them is one of the more frequently studied types of tasks in the investigation of perceptual learning. Yet, according to our criterion, this very specificity of the responses creates an apparent problem: if the responses are pre-established to the specific stimuli which are to be discriminated, these cannot, in themselves, be unfamiliar. But how can there be any room for learning, then? Indeed, the only way in which learning can enter into such a situation at all is through the presentation of familiar stimuli under conditions which make recognition difficult. But under these circumstances the problem of perceptual learning becomes a rather different one, involving factors which relate to the general perceptual conditions, or else to the identifying responses, rather than to the particular stimuli presented.

This point is well illustrated in some of the studies employing methods of "impoverished stimulation" to make recognition difficult (through exposure of stimuli tachistoscopically, in the periphery of the eye, under dim illumination, over a background of noise, etc.—cf. the review by E. Gibson [10]). Thus, in the study by Howes and Solomon (21) on the recognition of words in the tachistoscope, the words in the second half of the list were identified more readily than those in the first half, although the words in the two halves were not the same. This finding reflects thus a generalized practice effect, attributable to mechanisms of a peripheral nature perhaps, such as receptor adaptation or changes in focusing responses.

A factor of a rather different sort, relating to the response system, may also manifest itself in this type of situation. Where a considerable number of re-

sponse alternatives are available to the subject, these may not be equal in strength initially. Learning may then consist primarily in the gradual elimination of these preferential response habits. This appears to be the explanation, for instance, for at least part of the learning which occurred in Seward's study of letter recognition (31), in which subjects tended to limit their responses to but a portion of the alphabet at the beginning.

While one may devise ways of controlling for these extraneous factors (e.g., response strength can be equated by making all required responses available to the subject throughout the task), it is doubtful whether studies employing impoverished stimulation methods will yield much information of value to the study of perceptual learning. There are, however, a variety of other ways of making recognition difficult. For instance, the usual brightness relationships in the stimulus may be reversed, by the use of photographic negatives. Alternatively, objects may be presented from unfamiliar angles or in unfamiliar contexts, and, more particularly, in motion (cf. 14). If this kind of task gives rise to a learning process, it may be of considerable interest in its own right for the study of perceptual learning. But it is apparent that learning in a situation of this sort is not easily handled either in terms of the differentiation of variables of stimulation or in terms of associative processes. This type of task has, however, been investigated much too little to justify speculation as to the nature of possible learning effects.

THE PERCEPTUAL ASPECT OF PAIRED-ASSOCIATE LEARNING

In view of the important role often played by perceptual processes in the association of new responses to a set of stimuli, a criterion for the investigation of perceptual learning which seemingly

eliminates this problem from consideration may well be objected to as unduly restrictive. But it has been argued here only that it is not useful to study the paired-associate task itself as a perceptual learning situation, due to the confounding of perceptual and response variables. One may still apply the analytic framework developed in this paper to this problem, by examining the *results* of such a learning process with respect to the differentiation of stimuli that has taken place. This procedure may help, in fact, in clarifying some issues in the study of paired-associate learning.

Consider the work on the problem of stimulus predifferentiation, recently reviewed by Arnoult (4). This problem is concerned with the generally positive transfer effects obtained by establishing verbal responses to a set of stimuli, prior to the association of a different set of responses (typically motor) to the same stimuli. As Arnoult points out in his review, the nature of this phenomenon remains obscure, and there are at present not even sufficient grounds for choosing between a stimulus-differentiation interpretation and other alternative explanations which have been proposed to account for it. It seems plausible to relate this equivocal conclusion to the ambiguity inherent in the situation itself, with respect to the role of perceptual processes.

This role could be studied more directly by testing for the differentiation of the given set of stimuli on one or more relevant perceptual dimensions, before and after the association task. One would then be able to decide what has been learned about the stimuli during the course of the paired-associate learning, and thus to correlate the extent and kind of differentiation achieved with the learning in the transfer task.

The design just proposed actually resembles that used in the studies by

Arnoult (3) and Hake and Eriksen (18). Parts of these two studies concerned the effect of the association of letters of the alphabet to sets of nonsense shapes on the subsequent recognition of these shapes on a same-different basis. But this particular basis is not very revealing, since the mastery of the association task already implies that the stimuli have been differentiated from one another. The fact that in both studies the results were actually negative may thus seem rather puzzling—indeed, they have been cited as negative evidence on the question of stimulus predifferentiation. The paradox disappears, however, when we realize that in the recognition task of both studies new, arbitrarily chosen stimuli were introduced along with the old ones. Thus in order to expect transfer in this situation one would have to assume that the subjects had learned more about the training stimuli than was in fact required to differentiate them.

"EXTRINSIC" VS. "INTRINSIC" CUES IN PERCEPTUAL LEARNING

The approach to the problem of perceptual learning presented in this paper is intended chiefly for the analysis of restricted experimental situations, involving perceptual judgments under controlled practice. Yet some of the major issues in perceptual learning refer to the process by which the individual, during the course of his development, comes to perceive the world as he does. In fact, the contrast between "enrichment" and "differentiation" processes delineated by Gibson and Gibson appears to be directed at conceptions of perceptual development in this more general sense.

But since the acquisition of the responses to stimuli must form an integral part of the process of development, it becomes difficult in this case to isolate the perceptual aspect of what is termed "perceptual learning." Thus, insofar as

this long-term learning is concerned, the Gibsons' opposition between enrichment and differentiation may not be entirely warranted. There are undoubtedly two mutually dependent processes going on simultaneously: increasing differentiation of stimuli on the one hand, and development of intervening structures, such as proposed in "enrichment" theories to mediate the resulting discriminations, on the other hand.

Yet in one respect it seems possible to formulate a fairly sharp difference between the major empiricist interpretations of perceptual learning and the Gibsons' specificity theory. This difference, which parallels closely that proposed earlier between direct transfer and response association as bases for perceptual learning, concerns the type of features of the stimulus which the individual learns to discriminate.

One may distinguish, in effect, between those aspects of the stimulus which are extrinsic and those which are intrinsic to a particular judgment. The Gibsons are clearly concerned with the intrinsic stimulus correlates of perception—intrinsic in the sense that these aspects of the proximal stimulus invariably reflect the particular aspect of the physical environment which is being judged, by the very nature of the sensory apparatus and of the physical energies impinging on it. For example, the edges of a road seen from different heights will necessarily converge towards the horizon at differing angles (cf. 15, p. 140). Thus the angle of convergence constitutes an intrinsic correlate of perceived height. These correlates are therefore independent of experience with specific objects or in specific environments.

In contrast, the functionalists, as represented by the transactional school (22) or by Brunswik's probabilistic functionalism (8), do not differentiate between such intrinsic information and

that conveyed by objects or environmental features of a more extrinsic type with which the subject may have come into contact. Thus, for Brunswik, perception is a matter of the utilization of cues of varying degrees of "ecological validity," whatever their origin. Similarly, the assumptions of the transactionalists are based in part on experience with particular meaningful objects (e.g., the typical sizes of ping-pong and billiard balls [20]) and in part on experience with universal aspects of stimulation (e.g., the successive stimulation of adjacent points on the retina due to a moving object [34]).⁴

The question which emerges from these two opposing viewpoints is the following: are extrinsic and intrinsic cues functionally equivalent, or are there differences, quantitative or qualitative, in the roles played by these two kinds of information? With respect to the basic stimulus dimensions of the physical world, at least, such differences are surely not altogether implausible. It seems conceivable, at any rate, that the nature of the perceptual mechanism of the organism is such as to predispose him automatically to respond to information of the intrinsic type, on account of its very reliability.

In order to answer this question, the two types of cues must be put into conflict with each other, as in Slack's (32) study of the size estimation of normal, over-, and undersized chairs in the presence of the ordinary cues to distance. It

⁴ In Ames' (2) rotating trapezoidal window effect, both of these kinds of cues are invoked. There is, on the one hand, experience with windows and rectangular objects, and on the other hand, experience with perspective transformations in general. It is of interest that Allport and Pettigrew (1) have recently thrown doubt on the former of these experiential factors by showing that amount of experience with windows and rectangular forms is to a large extent unrelated to the perception of this illusion.

was found that the judgments were displaced somewhat towards the assumed size of a chair, although the true sizes of the stimuli accounted for the major portion of the variance in these judgments by far. But this study tells us little about the actual learning processes that may be reflected here, as we are only shown its end result.⁵

A more direct line of attack on this problem would be to juxtapose extrinsic and intrinsic cues in a perceptual learning experiment. Thorndike, in exploratory and rather sketchily documented work (33, pp. 77 ff.), has in effect employed this method, demonstrating, for instance, that through arbitrary association with an irrelevant cue (shape), non-veridical judgments of area could be induced. The relative importance of the two types of information could similarly be assessed by correlating them in a perceptual learning task, and rendering the extraneous cue irrelevant, or even reversing it, once the discrimination had been established. Of perhaps even greater interest might be the introduction into a perceptual learning experiment of a readily discriminated extraneous cue under probabilistic reinforcement. Subsequent tests with this cue removed would indicate to what extent the subject relies on highly visible though relatively unreliable cues, in

preference to possibly quite subtle but perfectly reliable sensory information. This kind of investigation might thus be expected to throw light on the relative merits not only of the enrichment and differentiation hypotheses of perceptual learning but also, by extension, of the psychophysical as compared to the functional approaches to perception generally.

Lastly, it may be noted that there is a definite connection between the notion of intrinsic and extrinsic cues and the two alternative interpretations of perceptual learning developed on the basis of our criterion. Once a response to a dimension has been established, the possibility for learning on a stimulus-transfer basis, without reinforcement, is dependent on the presence of intrinsic cues which are initially imperfectly differentiated. As for the associative type of learning, it is clearly based on the utilization of extrinsic cues. The postulation of these two types of cues appears, however, to be relevant mainly to discriminations along physical dimensions, while the distinction between the two varieties of perceptual learning may be applied more generally, e.g., to categorization learning (cf. the discussion of Type IV situations, above).

SUMMARY AND CONCLUSIONS

The primary aim of this paper has been to suggest a criterion for perceptual learning which will allow a conceptual distinction between learning based on perceptual functions and that based on response association. The approach suggested is to regard perceptual learning as the development of a generalization of a previously established differential response to a new stimulus. Thus defined, perceptual learning does not require the formation of new S-R associations and, in contrast to associative learning, may occur in the absence of external reinforcement.

⁵ This shortcoming applies more generally to the experimental work emanating from the empiricist position. Apart from some animal studies, few learning experiments have been carried out by these psychologists, as Gibson and Gibson (16) point out. Apparent exceptions to this statement are the recent studies by Kilpatrick (23) and Weiner (35) on learning in the distorted room, and—representing a somewhat different position—by Kohler (25) on adaptation to the wearing of reversing and distorting lenses. But these are really *unlearning* experiments, in which firmly established responses require modification due to altered stimulus conditions. They can thus not be assumed to represent the process of perceptual learning as it develops originally.

An examination of five types of judgments in the light of this definition brought out a number of specific testable problems with regard to the nature of perceptual learning, but the evidence concerning them is for the most part either unavailable or inadequate on methodological grounds. In several instances, the relevance of the "specificity hypothesis" advanced by Gibson and Gibson (16) was noted. This hypothesis, which describes "what is learned" in perceptual learning as an increased differentiation of variables of stimulation, may be of particular predictive value where the aspects of the stimulus which will be differentiated can be specified. Finally, an experimental approach to the Gibsons' opposition between "enrichment" and "differentiation" theories of perceptual learning was suggested, through the opposition of intrinsic and extrinsic cues in the learning of a discriminative response.

This paper, like that of the Gibsons, has sidestepped the question of the processes and mechanisms within the organism which mediate perceptual learning of the differentiation variety. Admittedly, from a theoretical point of view, the problem of perceptual learning is thus only half solved at best. The task remains to analyze the processes involved in terms of a set of constructs which will permit these phenomena to be integrated into a more general perceptual theory. It is possible that some constructs proposed in other contexts—e.g., Pitts and McCulloch's (29) "scanning mechanisms," or Piaget's (28) "decentrations"—may be adapted to account for improvement in the differentiation of stimuli. At the present, however, they remain largely speculative and of unknown value in the handling of perceptual learning phenomena.

In conclusion, it may be suggested that an analysis of the stimulus, aimed at specifying what aspects are discrimi-

nated as a result of learning, may itself contribute heavily to the eventual construction of adequate mediating mechanisms to explain perceptual learning.

REFERENCES

1. ALLPORT, G. W., & PETTIGREW, T. F. Cultural influence on the perception of movement: The trapezoidal illusion among Zulus. *J. abnorm. soc. Psychol.*, 1957, **55**, 104-113.
2. AMES, A. JR. Visual perception and the rotating trapezoidal window. *Psychol. Monogr.*, 1951, **65**, No. 7 (Whole No. 324).
3. ARNOULT, M. D. Transfer of pre-differentiation training in simple and multiple shape discrimination. *J. exp. Psychol.*, 1953, **45**, 401-409.
4. ARNOULT, M. D. Stimulus predifferentiation: Some generalizations and hypotheses. *Psychol. Bull.*, 1957, **54**, 340-351.
5. ATTNEAVE, F. Transfer of experience with a class-schema to identification-learning of patterns and shapes. *J. exp. Psychol.*, 1957, **54**, 81-88.
6. ATTNEAVE, F., & ARNOULT, M. D. The quantitative study of shape and pattern perception. *Psychol. Bull.*, 1956, **53**, 452-471.
7. BROWN, F. G., & ARCHER, E. J. Concept identification as a function of task complexity and distribution of practice. *J. exp. Psychol.*, 1956, **52**, 316-321.
8. BRUNSWIK, E. *Perception and the representative design of psychological experiments*. Berkeley: Univer. California Press, 1956.
9. GARNER, W. R., HAKE, H. W., & ERIKSEN, C. W. Operationism and the concept of perception. *Psychol. Rev.*, 1956, **63**, 149-159.
10. GIBSON, ELEANOR J. Improvement in perceptual judgments as a function of controlled practice or training. *Psychol. Bull.*, 1953, **50**, 401-431.
11. GIBSON, ELEANOR J., & BERGMAN, R. The effect of training on absolute estimation of distance over the ground. *J. exp. Psychol.*, 1954, **48**, 473-482.
12. GIBSON, ELEANOR J., BERGMAN, R., & PURDY, J. The effect of prior training with a scale of distance on absolute and relative judgments of distance over the ground. *J. exp. Psychol.*, 1955, **50**, 97-105.
13. GIBSON, ELEANOR J., & SMITH, JO ANN. The effect of training in distance esti-

- mation on the judgment of size-at-a-distance. Human Resources Research Center, Lackland Air Force Base. *Research Bull.*, 1952, No. 52-39.
14. GIBSON, J. J. (Ed.) *Motion picture testing and research*. Washington: U. S. Government Printing Office, 1947. (AAF Aviat. Psychol. Program Res. Rep. No. 7.)
 15. GIBSON, J. J. *The perception of the visual world*. Boston: Houghton Mifflin, 1950.
 16. GIBSON, J. J., & GIBSON, ELEANOR J. Perceptual learning: Differentiation or enrichment? *Psychol. Rev.*, 1955, **62**, 32-41.
 17. GIBSON, J. J., & GIBSON, ELEANOR J. What is learned in perceptual learning? A reply to Professor Postman. *Psychol. Rev.*, 1955, **62**, 447-450.
 18. HAKE, H. W., & ERIKSEN, C. W. Role of response variables in recognition and identification of complex visual forms. *J. exp. Psychol.*, 1956, **52**, 235-243.
 19. HARLOW, H. F. Primate learning. In C. P. Stone (Ed.), *Comparative Psychology*. (3rd ed.) New York: Prentice-Hall, 1951. Pp. 183-238.
 20. HASTORF, A. H. The influence of suggestion on the relationship between stimulus size and perceived distance. *J. Psychol.*, 1950, **29**, 195-217.
 21. HOWES, D. H., & SOLOMON, R. L. Visual duration threshold as a function of word-probability. *J. exp. Psychol.*, 1951, **41**, 401-410.
 22. KILPATRICK, F. P. (Ed.) *Human behavior from the transactional point of view*. Hanover, N. H.: Institute for Associated Research, 1952.
 23. KILPATRICK, F. P. Two processes in perceptual learning. *J. exp. Psychol.*, 1954, **47**, 362-370.
 24. KOEHLER, O. Vorsprachliches Denken und "Zählen" der Vögel. In E. Mayr & E. Schüz (Eds.), *Ornithologie als biologische Wissenschaft*. Heidelberg: C. Winter, 1949. Pp. 125-146.
 25. KOHLER, I. Ueber Aufbau und Wandlungen der Wahrnehmungswelt. *Oesterr. Akad. Wiss. Philos.-Histor. Kl.; Sitz.-Ber.*, 1951, **227**, 1-118.
 26. NEWMAN, S. E. Effects of contiguity and similarity on the learning of concepts. *J. exp. Psychol.*, 1956, **52**, 349-353.
 27. PASTORE, N. Discrimination learning in the canary. *J. comp. physiol. Psychol.*, 1954, **47**, 389-390.
 28. PIAGET, J., LAMBERCIER, M., BOESCH, E., & VON ALBERTINI, BARBARA. Introduction à l'étude des perceptions chez l'enfant et analyse d'une illusion relative à la perception visuelle de cercles concentriques (Delboeuf). *Arch. Psychol., Genève*, 1943, **29**, 1-107.
 29. PITTS, W., & McCULLOCH, W. S. How we know universals: The perception of auditory and visual forms. *Bull. math. Biophys.*, 1947, **9**, 127-147.
 30. POSTMAN, L. Association theory and perceptual learning. *Psychol. Rev.*, 1955, **62**, 438-446.
 31. SEWARD, J. P. The effect of practice on the visual perception of form. *Arch. Psychol.*, 1931, **20**, No. 130.
 32. SLACK, C. W. Familiar size as a cue to size in the presence of conflicting cues. *J. exp. Psychol.*, 1956, **52**, 194-198.
 33. THORNDIKE, E. L. *Human learning*. New York: Century, 1930.
 34. TOCH, H. H., & ITTELSON, W. H. The role of past experience in apparent movement: A reevaluation. *Brit. J. Psychol.*, 1956, **47**, 195-207.
 35. WEINER, M. Perceptual development in a distorted room: A phenomenological study. *Psychol. Monogr.*, 1956, **70**, No. 16 (Whole No. 423).

(Received December 30, 1957)

THE CONCEPT OF IDENTIFICATION¹

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Several years ago Sanford (20) presented an analysis of the concept of identification. In brief, Sanford suggested that the term be applied to situations in which "an individual may be observed to respond to the behavior of other people or objects by initiating in fantasy or reality the same behavior himself . . . the individual strives to behave in a way that is exactly like that of the object" (20, p. 109). Sanford further suggested that the motive for this imitative behavior was a threat to the person's self esteem. By limiting the term "identification" to those imitative behavioral sequences in which the motivation for the act was anxiety over self esteem, Sanford emphasized two points: (a) mere similarity in overt behavior between a subject and a model was not necessarily a measure of identification, and (b) the motive for the imitative behavior was one of the defining characteristics of an identificatory response.

The various behavioral phenomena which have been labeled "identification" differ in their manifest properties and motivations. The following four classes of behavior have been described as related to the process of identification because they all can lead to similarities in behavior between a subject and a model.

¹ This research was supported, in part, by a research grant (M-1260) from the National Institute of Mental Health of the National Institutes of Health, United States Public Health Service. The views of Wesley Allin-smith, Vaughn J. Crandall, Leonard M. Lansky, and Howard A. Moss are especially acknowledged. A major stimulus for the present essay was a workshop in parent-child relations supported by USPHS Grant 1649 and held at the Merrill-Palmer School, Detroit, Michigan, July 14-27, 1957.

Imitation Learning

This term refers to the initiation and practice of certain responses (gestures, attitudes, speech patterns, dress, etc.) which are not subject to prohibition by the social environment and which are assumed to be the result of an attempt to imitate a model. The behavior has been labeled either "matched-dependent behavior" or "copying" by Miller and Dollard (17). Miller and Dollard posit that initially the imitative act occurs by chance and the act can only be reinforced if some drive is reduced following the execution of the response. According to this view only direct reward from the social environment, like praise or affection, can strengthen the person's tendency to imitate a model. Mowrer (18) distinguishes between developmental and defensive identification. In the former process, the person imitates or reproduces the behavior of a model in order to "reproduce bits of the beloved and longed-for parent" (18, p. 615). Mowrer suggests that most imitation of a model is the result of the desire to reproduce responses which have acquired secondary reward value through association with a nurturant and affectionate model. Thus, Mowrer emphasizes the self-rewarding aspect of certain imitative acts as opposed to Miller and Dollard's emphasis on direct reward from the social environment.

Prohibition Learning

This term refers to the adoption and practice of the prohibitions of the parents and parent substitutes. The acquisition of these prohibitions bears some relation to the process of super-ego development as described by psy-

choanalytic theory (2, 3, 4, 11). Several investigators have suggested that a major motivation for the acquisition of some prohibition is anxiety over anticipated loss of love (10, 11, 18, 20, 23). Sanford labeled this process "introjection" and suggested that the learning and maintenance of this class of behavior might be explained without use of the concept of identification.

Identification with the Aggressor

This phrase refers to the adoption of behaviors which are similar to those of an aggressive or threatening model. The motivation for this "imitation" is assumed to be anxiety over anticipated aggression or domination by the threatening model. It is difficult to explain this behavior as a product of either prohibition or imitation learning, since the motive and reinforcement do not seem related to anxiety over anticipated loss of love or desire for a direct, social reward like praise or affection. Anna Freud (2) has labeled this phenomenon "identification with the aggressor," Mowrer has called this process "defensive identification" (as distinct from developmental identification), and Sanford has suggested that the term "identification proper" be restricted to this class of behavior.

Vicarious Affective Experience

This phrase refers to the experience of positive or negative affect on the part of a person as a result of an event which has occurred to a model. Salient examples of this phenomenon are (a) a child's elation or depression at learning that his parent is a success or failure, or (b) a mother's elation following the success of her child in school. This phenomenon of vicarious, affective experience has been attributed to a person's identification with a model, but this affective response has been difficult to explain and often neglected by psy-

chologists investigating the identification process. These four phenomena (imitation learning, prohibition learning, identification with the aggressor, and vicarious, affective experience) appear to be mediated by different motives and rewards, and an analysis of each of them is one purpose of this paper.²

In different contexts, social scientists have used the term "identification" to refer to three different sets of variables: (a) the process of identification; (b) individual differences in the content of the behaviors, motives, and attitudes acquired as a result of the identification process; and (c) the differential effect of various models that are used during the identification process (3, 4, 5, 7, 9, 11, 13, 15, 16, 25, 26). This paper recognizes the relevance of the model and content dimensions but is primarily concerned with the process of identification, and will attempt to analyze this process in behavioral terms. It is suggested that the process remains the same regardless of the models used or the specific behavioral content that is acquired as a result of an identification.

Definitions of Identification

The concept of identification originated in psychoanalytic theory, and Freud made a distinction between primary and secondary identification (3, 4, 5). Primary identification referred to the initial, undifferentiated perception of the infant in which an external object was perceived as part of the self, while secondary identification began after the child had discriminated a world of ob-

² In an unpublished paper presented at a symposium at Harvard University in 1957, Bronfenbrenner described three types of identification: (a) anaclitic identification, (b) identification with a source of power, and (c) identification through reinforcement of a role model. These three terms are similar in meaning to the present phrases of prohibition learning, identification with the aggressor, and imitation learning, respectively.

jects separate from the self. Freud implied in his later writings that the process of secondary identification was motivated primarily by the motives and anxieties created by the oedipal situation. In order to reduce the anxiety over anticipated aggression or rejection from the same-sex parent and obtain vicariously the affection of the opposite-sex parent, the child identified with the former. Identification was described by Freud as "the endeavor to mould a person's own ego after the fashion of one that has been taken as a model" (5, p. 63).

Mowrer's concept of "defensive identification," Sanford's definition of "identification proper," and Anna Freud's description of "identification with the aggressor" are all related to the earlier psychoanalytic hypothesis that the threat value of the same-sex parent motivated the child to identify with him in order to reduce the anxiety associated with this threat. However, it is suggested that an individual may identify with a model not only to reduce anxiety over anticipated aggression from a model but also to experience or obtain positive goal states which he perceives that the model commands. The thesis of this paper is that the motivation to command or experience desired goal states of a model is salient in the development and maintenance of an identification.³ It will be suggested later that two major goal states involved in identification behavior are (a) mastery of the environment and (b) love and affection. However, it is not implied that these

are the only goals which an individual desires to command.

Definition

Identification is defined as an acquired, cognitive response within a person (S). The content of this response is that some of the attributes, motives, characteristics, and affective states of a model (M) are part of S's psychological organization. The major implication of this definition is that the S may react to events occurring to M as if they occurred to him.

The Acquisition and Maintenance of an Identification

Although identification has been defined as a cognitive response, it is not implied that the content of the response is available to consciousness or easily verbalized. Thus the terms "cognitive response," "belief," "wish," or "assumption" will be used in this text to include cognitive processes not always available through verbal report. Identification is not viewed as an all-or-none process. Identification is a response that can vary in strength and there will be differences in the degree to which an S believes that the characteristics of a model, whether assets or liabilities, belong to him. In addition, the S may become identified, to differing degrees, with a variety of models. The motives and reinforcements that are involved in the acquisition and maintenance of this cognitive response are elaborated in the following assumptions.

Assumption 1

Initially the S perceives that the M possesses or commands goals and satisfactions that the S desires. This perception leads to a wish to possess these desired goal states.

Assumption 2

The wish to command the goal states of the M leads to the desire to possess

³ It is assumed that anticipation of a positive goal state is associated with the anticipation of a change in affect, and thus the phrase "experience goal states of the model" will be used synonymously with the phrase "experience affective states of the model." This assumption agrees with McClelland's definition of a motive as an "anticipation of a change in affective state" (14, p. 466).

the characteristics of the M because S believes that if he were similar to the M he would command the desired goals. That is, the S assumes that the more similarity there is between the S and M the more likely S is to possess or command the desired goal states of the M.

To illustrate, let the S be a child and the M a mother, although S and M could be an adolescent boy and the leader of a group, or a girl and her older sister. The child perceives that the mother can feed the child, restrict the child, obtain articles out of the child's reach, punish the child, etc. Thus, to the S, the M appears to command desired skills and goal states. The discrepancy between the child's perception of his inability to obtain these desired goals and his perception of the more adequate adult elicits the wish to possess or control those goals which he perceives that M commands. The perceptions of the child are subject to distortion, and the child may exaggerate the degree to which M commands desired goals. It was assumed (Assumption 2) that the wish to command these goal states led to the expectation that if S possessed M's characteristics he would also command these desired goals. There often is direct reinforcement of the belief that to "be similar to" a model is equivalent to possessing his positive attributes. Often, the social environment tells the child directly that he is similar to a parent in certain characteristics, and this communication may be contiguous in time with statements related to some of the model's desired goal states. For example, parents and relatives may tell the child "You have your father's eyes," and often add, "You'll grow up to be big and strong just like Daddy." It is suggested that these statements which associate similarities in external attributes with command of desired goal states have an important effect on the child's learning about him-

self, and lead the child to the expectation that to be similar to the model is equivalent to possessing his positive and desirable attributes.

Assumption 3

The identification response (i.e., "some of the characteristics of the model are mine") is reinforced each time S perceives or is told that he is similar to the M. One type of reinforcement for the identification response occurs when an S is told directly that he and the M are similar in temperament or appearance. It is suggested that a second type of reinforcement for this cognitive response is S's own perception of similarity to the M. Once again, consider the case of the small child and his parent. Although the child may perceive marked differences in size, strength, and skills between himself and the M, he may perceive a similarity in affective states, such as joy, anger, or sadness. The importance of the perception of similarities in affective states between the S and M is stressed because a major motive for identification is a desire to experience positive affective states of the model. Thus, perception of similarity in affect is assumed to have saliency as a reinforcement. If the parent becomes angry, sad, or happy and communicates these affects to the child, the child has the opportunity to perceive that he and the M experience similar feelings. This perception reinforces the belief that there is similarity between the S and M. In addition to similarity in affective states, perception of similarities in external characteristics will reinforce the identification response. With specific reference to the child-parent relation, it is assumed that perception of similarities in sexual anatomy, dress, amount and distribution of hair, and other external attributes are potential reinforcements of the identification. Thus, while the identification response is be-

ing learned, the major reinforcements for the response are perceptions of similarity between the S and M.⁴ Freud suggested that perceptions of similarity strengthen an identification, for he wrote,

Identification . . . may arise with every new perception of a common quality shared with some other person who is not an object of the sexual instinct. The more important this common quality is, the more successful may this partial identification become, and it may thus represent the beginning of a new tie (5, p. 65).

Assumption 4

In order for the identification belief to be maintained, the S must not only perceive similarity between the S and M but he also must experience some of the desired, affective goal states of the M. Thus, if the M were successful or happy and S believed that M was experiencing positive affect, the S would also feel positive affect appropriate to the success, and this experience would reinforce his identification. The S also may experience affect appropriate to events occurring to M as a result of the expectation that the social environment will respond to him the same way it responds to the M. That is, when the S has developed some degree of identification with the M he may anticipate that when the social environment praises or rewards the M, it will behave similarly to him. If, on the other hand, the M were sad or criticized, S might experience

negative affect because of the identification belief that he and the M were similar and the expectation that the environment might react to him as it did to M. However, if no vicarious command of desired goals or positive affect were experienced as a result of the identification, then the response should extinguish just as any other habit does in the absence of positive reinforcement.⁵ That is, some degree of identification should be maintained as long as S perceives that the M commands desired goals. When the S no longer perceives the M in this fashion, then both the motivation for the identification and the intensity of the positive reinforcement should decrease.

The Acquisition of Behavior Similar to a Model: The Motives for Imitation, Identification, and Prohibition Learning

Since perceptions of similarity between the S and M reinforce the identification response, the S may imitate the M during the acquisition phase of an identification in order to increase the degree of similarity. It is acknowledged that the social environment rewards imitative behaviors with affection and praise, and these direct, social reinforcements may strengthen the tendency to imitate adults independently of any identification motives. However, it is suggested, along with Sears et al. (23), that direct, social reinforcement of imitative behavior cannot account for all of the imitative responses that the S initi-

⁴ It is suggested that the concept of identification has not yielded to a behavioral analysis because the notion of social reinforcement has been viewed as a specific action directed at an individual by a reinforcing agent. There has been a tendency to overlook the possibility that a perception, fantasy, or thought may be a potential reinforcement of a response. A recent experimental finding by Estes and Johns (1) supports the hypothesis that a person's perception of a situation, even though objectively inaccurate, can reinforce his subsequent behavior.

⁵ This view of identification suggests a measurement operation which differs from the usual practice of assessing similarities in behavior between an S and an M. One measure of degree of identification would be the degree to which an S's affective state or behavior was influenced as a result of events that occurred to an M. That is, praise or criticism of an M in S's presence should lead to corresponding changes in the affective state of an S who was identified with the M.

ates. A four-year-old child may simulate adult behaviors when the child is alone or in situations where the parents discourage or punish the imitative response. However, despite the punishment or absence of social reward for some imitative behaviors, the behavior continues to be practiced. Sears et al. call this behavior "role practice" and assume that it is motivated by the "desire to reproduce pleasant experiences" (23, p. 370). Consider the three-year-old girl who plays the role of mother alone in her room. It is hypothesized that a potential reinforcement for this behavior is the creation, in fantasy, of perceptual similarity between the behaviors of the S and M. This perception strengthens S's identification with the M and allows S to share vicariously some of the positive goal states which M commands.

A somewhat different phenomenon is the behavior called "identification with the aggressor" by A. Freud or "defensive identification" by Mowrer. Anna Freud describes a girl who was afraid of ghosts and suddenly began to make peculiar gestures as she ran in the dark. She told her brother, "there is no need to be afraid, you just have to pretend that you're the ghost who might meet you" (2, p. 119). The present theory assumes that the child desired the threatening power of the feared object and this motive elicited the imitative behavior. The fantasied perception of similarity to the feared model gave S a vicarious feeling of power and reduced her anxiety over attack. It is suggested that "identification with the aggressor" does not differ from other identification responses with respect to the basic mechanism of acquisition but does involve a specific motive and goal state. Identification with the aggressor involves a specific relationship between the S and M in which S fears the M. Thus, S desires the aggressive power or

threat value of the M in order to reduce his own anxiety over anticipated attack. It may be misleading to classify "identification with the aggressor" as qualitatively different from other identificatory behavior merely because the motive and goal differ from those involved in other identifications.

A third motive which can lead to behavioral similarity between an S and M is anxiety over anticipated loss of love or nurturance. It is suggested that many social prohibitions which the M practices are learned by the S in situations in which this anxiety motivate the acquisition and maintenance of the response. The reinforcement for the learned prohibition is continued acceptance and a consequent reduction in anxiety over rejection. The research of Sears et al. (23) suggests a relationship between "high conscience" in a child and a pattern in which the mother is nurturant and uses withdrawal of love as a disciplinary technique. In summary, any one response which is imitative of a model may be mediated by three different motive-reinforcement sequences, and in many instances all three may be involved in producing behavioral similarity between an S and M.⁶ Thus, "eating neatly," "getting good grades," or "being nonaggressive" could be motivated by the desire for praise as in imitation learning, by anxiety over loss of love as in prohibition learning, or by the desire to create perceptual similarity between the S and M as in identification. Thus, mere similarity in overt behavior between an S and M may not be the most sensitive measure of degree of identification.

At a more speculative level, it is sug-

⁶ In a manuscript being prepared for publication, H. Kelman suggests that the response of conformity to the attitudes of another person can be mediated by three different motives. His analysis of conformity parallels the present discussion of imitative behavior.

gested that the behaviors which have been called "self actualizing" (6) could be motivated and reinforced by a desire for perceptual similarity to an M and be an indication of early identification tendencies. Even the most orthodox supporters of the importance of simple imitation learning find it difficult to explain the child's initial imitations of a model. Once the child has begun to imitate a model it is likely that praise and recognition from adults could maintain this behavior. However, why does the child suddenly want to dress himself, sit on the toilet alone, or put on Daddy's shoes? It is difficult to account for the initial display of this imitative behavior, and the term "self actualization" implies that the child has some biological drive to use his potentialities. This hypothesis seems no more parsimonious than the suggestion that the initiation of these "self actualizing" behaviors is motivated by S's desire to create perceptual similarity between himself and a model.

*Two Goals Motivating Identification:
Mastery and Love*

It has been assumed that S's desire to command certain goal states motivates his identification with a model. It is suggested, for the child especially, that two important goal states that the S desires to command are (a) a feeling of power or mastery over the environment and (b) love and affection. Attainment of these goals should lead to diminution in anxiety over helplessness or loneliness. The young child perceives that he is not able to gratify all of his needs while the parental model is perceived as more capable of dealing with the environment. This discrepancy between the S's perception of his own relative helplessness and the power that he perceives M to possess motivates the wish to have M's power and the search for percep-

tions of similarity between himself and the M.

Unfortunately, there are no empirical studies which directly test these hypotheses because most of the research on identification has used similarities in behavior between an S and M as the measure of identification. However, there are some results which are at least consistent with the view that the child identifies with the more powerful parent and the one who is perceived to command important sources of gratification. Payne and Mussen (19) reported that adolescent boys who perceived the father as rewarding on projective tests were more highly identified with the father (based on similar answers to a personality inventory) than boys who pictured their fathers as non-rewarding. In addition, boys with dominant and "masculine" mothers tended to be poorly identified with the father. P. S. Sears (22) reported a finding that is more difficult to explain without use of the concept of identification. She found, in a doll-play situation, that kindergarten girls used the mother doll as agent significantly more often than the father doll, while boys used both mother and father dolls with more nearly equal frequency. Since the mother is initially the major controller of gratifications for both sexes, one might expect an initial identification with her for both boys and girls. P. S. Sears (22) also reports that the kindergarten boys who used the mother doll most often had mothers who were (a) more nurturant than the father, (b) more critical of the father, and (c) more restrictive of the child's mobility outside the home. This result is consistent with the hypothesis that the child is predisposed to identify with the parental model who is perceived as controlling important goal states.

A study of Maccoby and Wilson (15) furnishes more direct support for the

present hypotheses. The authors showed movies to seventh grade boys and girls and then determined the protagonist with whom the child identified. The most significant result was that a "boy's choice of screen character (the one with whom he was presumed to identify) is more closely related to the social class level to which he aspires than to the level his family currently occupies" (15, p. 79). This result suggests that the child identified with models who commanded desired goals.

A second goal state which may motivate identification is the desire for nurturance and affection. In addition to Freud's classical hypothesis that the child identified with the same-sex parent in order to receive vicariously the affection of the opposite-sex parent, there are situations in which nonparental models command sources of affection. The relation between siblings is such a situation, and the younger child may identify with an older sibling if the former perceives that the latter commands parental affection. The research of Helen Koch (12) indirectly supports this hypothesis. She reported that school-age boys with older sisters tended to develop more feminine attributes than boys with older brothers. On the other hand, girls with older brothers tend to be more masculine than girls with older sisters. In the experiment of Maccoby and Wilson, described earlier, the authors reported that girls were more likely than boys to recall movie content involving boy-girl interaction while boys were superior on recall of aggressive acts by the hero. If one assumes that the need for affection is stronger for girls than for boys, and that the recalled content is influenced by the model chosen for identification, then these results suggest that the specific goal states desired by the S determine the models chosen for identification.

Factors Influencing the Strength of Identification

The strength of the identification habit, following a basic behavioral law, should be a function of the strength of the motive and the quality and frequency of the reinforcement (8). It would be predicted, therefore, that the most intense identification would occur when the S had strong needs for love and power, felt incapable of gratifying these motives through his own skills, and perceived similarity between himself and an M who commanded these goals. Utilizing this hypothesis, two generalized predictions can be made concerning the strength of identification for different ages and models.

1. The strength of identification tendencies should decrease with age because, in general, the individual's ability to gratify his needs for mastery and love through his own behavior, rather than through a vicarious mechanism, should increase with development. Thus, the identifications of a young child should be more intense than the identifications of older individuals.

2. An identification with an M with whom S was in direct contact should be stronger than with an M with whom S was not in contact, assuming that the motivation for identification was constant and the models were perceived as equally potent. This statement is based on the assumption that the reinforcements of perceived similarity are stronger when S perceives the affects and attributes of the M directly as opposed to instances in which he is merely told that he is similar to the M. Thus, degree of identification with a father with whom S was in contact should be greater than with an imagined fantasy father whom S had never seen. Only very indirect evidence is available to support this prediction. However, reports by P. S. Sears (21) and Sears

et al. (24) suggest that absence of the father from the home tends to decrease the degree of "masculine" doll play in preschool boys while this experience has little effect on the doll play of girls. The results are open to alternative interpretations but are not inconsistent with the present hypothesis.

SUMMARY

This paper has attempted to analyze the concept of identification and place the concept within a learning-theory framework. Identification was defined as an acquired, cognitive response. The content of this response was that some of the characteristics of a model belonged to the individual and the individual behaved as if some of the characteristics and affective states of the model belonged to him. Identification was not viewed as an all-or-none process. An identification can vary in strength and the individual can identify, to differing degrees, with a variety of models. The motive for the acquisition and maintenance of the identification response was a desire for the positive goal states commanded by the model, and mastery of the environment and love-nurturance were suggested as two important goals. The reinforcement for the acquisition of the identification was perceived similarity in attributes between the person and the model. Thus, the person may strive to imitate aspects of the model's behavior in order to create perceptual similarity between himself and the model. Once the identification was established, the individual behaved as if the goal states of the model belonged to him and the positive affect derived from this vicarious sharing of desired goal states helped to maintain the identification.

It was suggested that the usual emphasis on similarities in overt behavior between an individual and a model is not the best measure of identification,

since the motives and reinforcements involved in imitation and prohibition learning could also explain similarities in behavior between two people. A differentiation of imitative behavior based on imitation learning, prohibition learning, and identification was attempted.

REFERENCES

1. ESTES, W. K., & JOHNS, MARCIA D. Probability learning with ambiguity in the reinforcing stimulus. *Amer. J. Psychol.*, 1958, **71**, 219-228.
2. FREUD, ANNA. *The ego and the mechanisms of defense*. London: Hogarth, 1937.
3. FREUD, S. *New introductory lectures in psychoanalysis*. New York: Norton, 1933.
4. FREUD, S. *The ego and the id*. London: Hogarth, 1935.
5. FREUD, S. *Group psychology and the analysis of the ego*. London: Hogarth, 1949.
6. GOLDSTEIN, K. *The organism*. New York: American Book, 1939.
7. GRAY, SUSAN W., & KLAUS, R. The assessment of parental identification. *Genet. Psychol. Monogr.*, 1956, **54**, 87-114.
8. HULL, C. L. *Principles of behavior*. New York: Appleton-Century-Crofts, 1943.
9. KAGAN, J. The child's perception of the parent. *J. abnorm. soc. Psychol.*, 1956, **53**, 257-258.
10. KAGAN, J. Socialization of aggression and the perception of parents in fantasy. *Child Developm.*, 1958, **29**, 311-320.
11. KNIGHT, R. P. Introjection, projection and identification. *Psychoanal. Quart.*, 1940, **9**, 334-341.
12. KOCH, HELEN L. Attitudes of young children toward their peers as related to certain characteristics of their siblings. *Psychol. Monogr.*, 1956, **70**, No. 19 (Whole No. 426).
13. LAZOWICK, L. M. On the nature of identification. *J. abnorm. soc. Psychol.*, 1955, **51**, 175-183.
14. MCCLELLAND, D. C. *Personality*. New York: Sloane, 1951.
15. MACCOBY, ELEANOR E., & WILSON, W. C. Identification and observational learning from films. *J. abnorm. soc. Psychol.*, 1957, **55**, 76-87.
16. MARTIN, W. E. Learning theory and identification: III. The development of

- value in children. *J. genet. Psychol.*, 1954, **84**, 211-217.
17. MILLER, N. E., & DOLLARD, J. *Social learning and imitation*. New Haven: Yale Univer. Press, 1941.
18. MOWRER, O. H. *Learning theory and personality dynamics*. New York: Ronald, 1950.
19. PAYNE, D. E., & MUSSEN, P. H. Parent-child relations and father identification among adolescent boys. *J. abnorm. soc. Psychol.*, 1956, **52**, 358-362.
20. SANFORD, R. N. The dynamics of identification. *Psychol. Rev.*, 1955, **62**, 106-118.
21. SEARS, PAULINE S. Doll play aggression in normal young children: Influence of sex, age, sibling status, father's absence. *Psychol. Monogr.*, 1951, **65**, No. 6 (Whole No. 323).
22. SEARS, PAULINE S. Child rearing factors related to playing sex-typed roles. *Amer. Psychologist*, 1953, **8**, 431. (Abstract)
23. SEARS, R. R., MACCOBY, ELEANOR E., & LEVIN, H. *Patterns of child rearing*. Evanston, Ill.: Row, Peterson, 1957.
24. SEARS, R. R., PINTLER, MARGARET H., & SEARS, PAULINE S. Effect of father separation on pre-school children's doll play aggression. *Child Developm.*, 1946, **17**, 219-243.
25. SEWARD, J. P. Learning theory and identification: II. The role of punishment. *J. genet. Psychol.*, 1954, **84**, 201-210.
26. STOKE, S. M. An inquiry into the concept of identification. *J. genet. Psychol.*, 1950, **76**, 163-189.

(Received February 27, 1958)

INTRALIST GENERALIZATION IN PAIRED-ASSOCIATE LEARNING¹

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It seems to be generally accepted that, during the course of paired-associate learning, generalization tendencies increase to a maximum and then decrease. This hypothesis is endorsed by Osgood (16, pp. 537-539), and stated without comment by Bugelski (3, p. 410), Hovland (12, p. 666), and Underwood (17, p. 408). This increase and decrease in generalization tendencies during verbal learning was first suggested by Gibson (8), who made it an integral part of her theoretical formulation. Her Postulate 4 states, "Generalization will increase to a maximum or peak during the early stages of practice with a list, after which it will decrease as practice is continued" (8, p. 206). In this article we would like to suggest an alternate but opposing hypothesis, that generalization is greatest at the start of paired-associate learning and only decreases with practice.

In a paired-associate list n pairs long, four different types of responses can be distinguished: correct responses ($S_a-R_a, S_b-R_b, \dots, S_n-R_n$); incorrect responses ($S_a-R_b, S_a-R_c, \dots, S_a-R_n; S_b-R_a, S_b-R_c, \dots, S_b-R_n$, etc.); intrusions (any responses other than R_a, R_b, \dots, R_n); and omissions (no response). Generalization is "... the tendency for a response R_a learned to S_a to occur when S_b (with which it has not been previously associated) is presented" (8, p. 204). According to this definition,

an incorrect response would be a generalized response, as both are essentially the right response to the wrong stimulus. An intrusion would not be a generalized response to any stimulus in the list because it is not a correct response to any stimulus in the list. And obviously correct responses and omissions are not generalized responses. Therefore, it would seem (and this is the position taken here) that in paired-associate learning incorrect responses and generalized responses are one and the same.²

On a common-sense level it seems unreasonable to consider errors early in learning as generalized responses. If the correct response has not been learned (i.e., if S has not yet learned S_a-R_a), how could an incorrect response indicate generalization? Also, the definition of generalization given above states that the response R_a must be learned to the stimulus S_a before S_b-R_a could be considered a generalized response. However, if the response R_a does in fact occur to stimulus S_b , this would almost of itself indicate that some learning had already occurred; the probability of the occurrence of this response (or for that matter any other response on the list) by chance is so slight as to be negligible. Therefore, it would seem quite unlikely that an incorrect response could occur before some learning had taken place.

Equating incorrect and generalized responses is desirable from the point of

¹ This work was supported by a grant, G-2590, from the National Science Foundation. The author would like to thank Spaulding Rogers, Norman J. Slamecka, and Ernest N. Damianopoulos for a critical reading of the manuscript.

² An incorrect response could also indicate response generalization. However, in this article it is assumed that the response terms are sufficiently dissimilar and/or predifferentiated so that no response generalization occurs; all generalization will therefore be considered stimulus generalization.

view of simplicity. The definition of a generalized response in paired-associate learning becomes clear cut and unambiguous when the two are considered synonymous. There is no need to set up some arbitrary point in the learning before which incorrect responses would not be generalized responses but after which incorrect responses would be generalized responses. And, as will be pointed out in the final section of this paper, this position results in a prediction about transfer of training which is apparently substantiated by the available data. Therefore, it is felt that incorrect responses at any stage in paired-associate learning should be considered generalized responses.

In the standard paired-associate learning task S is not informed as to what the responses are before he actually starts to learn the list. Therefore, he must learn that the responses are R_a, R_b, \dots, R_n . This may involve learning to make responses that have never previously been made, and it will certainly involve learning to restrict the range of responses to R_a, R_b, \dots, R_n . Also, S must learn the pairings. That is, he must learn $S_a-R_a, S_b-R_b, \dots, S_n-R_n$. Furthermore, within limits he is free to respond or not as he wishes. Therefore, on the first trial of a paired-associate learning task which uses the anticipation method, the probability of an incorrect or generalized response is very slight. If S learns to a criterion of so many perfect trials, the probability of an incorrect or generalized response at the end of learning is also very slight. Assuming that some generalization occurs, there obviously must be first an increase, then a decrease in the frequency of incorrect or generalized responses. This does not mean, however, that there is an increase and then a decrease in generalization, for under these conditions the frequency of incorrect or

generalized responses is not an appropriate measure of generalization.

The frequency of incorrect responses is an inappropriate measure of generalization because omissions can occur, and they introduce a systematic bias. Omissions are most frequent at the start of learning, yet this is exactly the time when the frequency of generalized responses is low. If omissions were prohibited and S forced to respond to each stimulus, almost certainly most of the responses (excluding for the moment intrusions) would be incorrect or generalized responses, not correct responses. Therefore, contrary to what Gibson assumed (9, p. 194), the frequency of overt generalized responses is not a satisfactory measure of generalization tendencies when omissions are possible, for omissions result in a serious underestimation of the amount of generalization that would occur were S forced to respond.

Intrusions introduce a further complication; they too, if allowed to occur, would be most frequent early in learning. In the standard paired-associate procedure S must learn both the responses and the pairings, and intrusions indicate that S has not yet learned the responses perfectly. But, as Hovland and Kurtz (13) have pointed out, this is a separate problem from the learning of the pairings (or, in the case they discuss, the order in a serial list). If the chief purpose is to study generalization, the responses are merely a means to an end; one draws conclusions about confusion among the stimuli from the responses they evoke. Therefore, learning the responses merely introduces a complication that should be avoided.

It is felt, then, that to study generalization tendencies during learning both omissions and intrusions should be ruled out. This would not be particularly difficult to do. To exclude omissions, forced anticipation could be used;

S would be instructed always to respond during the "stimulus-only" period, even if the response were a pure guess. To exclude intrusions, *S* would be told the responses (though naturally not the pairings) in the instructions. If the responses were unfamiliar, a familiarization procedure such as Hovland and Kurtz (13) used could precede the learning. Under these conditions the frequency of incorrect or generalized responses would be an adequate measure of generalization tendencies, since omissions and intrusions had been ruled out.

Under these conditions only two types of responses would be possible, correct responses and incorrect or generalized responses. Assuming that *S* followed instructions and assuming that learning occurred, the frequency of correct responses would (disregarding inversions and plateaus) increase regularly with practice. Concomitantly, the frequency of incorrect or generalized responses would decrease regularly with practice. And, since it is felt that these are the appropriate conditions under which to measure generalization, it would seem that generalization tendencies have to be at a maximum at the start of learning and decrease with practice. There is, therefore, only a decrease, and no increase, in generalization during paired-associate learning.

The foregoing has been based on the assumption that the similarity among stimuli is the same. That is, no two stimuli are more similar than any other two. This is probably impossible to achieve in practice, and is often deliberately violated in order to get groups of stimuli within the total list with high intragroup similarity. If *S_a* and *S_b* are very similar to one another, the probability of *S_b*—*R_a* may be greater after a certain amount of practice than it was originally. However, the fact that the probability of this one specific generalized response has increased does not

mean that the probability of all generalized responses to *S_b* has increased. At the start of learning (assuming no omissions or intrusions), the probability that *S_b*—*R_b* will occur by chance would be $1/n$, the probability that *S_b*—*R_a* will occur by chance would also be $1/n$, and the sum of the probabilities that other incorrect or generalized responses will occur by chance would be $(n-2)/n$. Even though the probability of *S_b*—*R_a* may have increased with practice (and it may increase appreciably), provided that the probability of the correct response *S_b*—*R_b* does not drop below chance (i.e., $1/n$), then this increase must be accompanied by a corresponding decrease in the probability of other incorrect or generalized responses (i.e., $(n-2)/n$). Therefore, under these conditions there can be no net increase in generalization.

Suppose, however, that the probability of the correct response *S_b*—*R_b* did actually drop below chance ($1/n$). This could conceivably occur if *S_a*—*R_a* was learned (i.e., if the probability of *S_a*—*R_a* increased from $1/n$ to some value *X*, where $1/n < X \leq 1.00$), if *S_b*—*R_b* was not learned, and if *S_a* and *S_b* were sufficiently similar (in the extreme case indistinguishable) to produce generalization. Assume that there is a decrease in the probability of *S_b*—*R_b*; the maximum decrease possible would be from $1/n$ to 0, or $1/n$. At the same time there must, of necessity, be an increase in the probability of a correct response to *S_a*. This increase would be from $1/n$ to *X*, or $X - 1/n$. The only way in which there could be a net increase in generalization tendencies would be for the decrease in the probability *S_b*—*R_b* to be greater than the increase in the probability *S_a*—*R_a*; i.e., for $1/n$ to be greater than $X - 1/n$. For this to occur, *X* would have to be smaller than $2/n$, or the increase in the probability of the correct response

S_a-R_a would have to be less than $1/n$. With a paired-associate list of any appreciable length this maximum allowable increase in probability would be so slight as to make it doubtful that any generalization could occur after so little learning. So the conditions necessary for a net increase in generalization would seem to rule out the possibility that any generalization will occur.

It is possible, of course, for more than two stimuli to be similar. If two or more stimuli are similar to S_a , the maximum allowable increase in the probability of S_a-R_a becomes n'/n , where n' is the number of stimuli similar to S_a . The net increase in generalization then becomes $n'/n - (X - 1/n)$, or $(n' + 1)/n - X$. This is the most general expression, and, as before, the smaller X is, the more generalization tendencies could increase but the less likely it is that any generalization could occur. However, as X approaches 1.00, a net increase in generalization becomes impossible no matter how many stimuli are similar to S_a . So, to summarize, because the increase in the probability of one specific generalized response must be accompanied either by a decrease in the probability of other generalized responses to the same stimulus or by the increase in the probability of a correct response to a similar stimulus, it seems highly unlikely that, even with unequal intralist stimulus similarity, there ever could be a measurable increase in generalization tendencies with practice.

The hypothesis presented here suggests that there is only a decrease in generalization tendencies, yet studies of conditioning have shown an initial increase. One explanation for this difference is that in paired-associate learning all stimuli are presented to S from the first trial on. Learning requires the formation of discriminations, and generalized responses must be extinguished if a criterion of learning is to be reached.

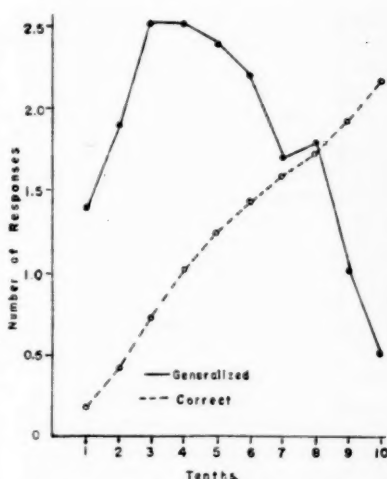


FIG. 1. Curve for number of correct and generalized responses during learning (copied from Gibson's Fig. 3 [9, p. 195]).

On the other hand, in studies of classical conditioning the stimuli used in the test for generalization are customarily not presented until the conditioned response has become established, and there is no opportunity for prior extinction of generalized responses.

RELATED STUDIES

At least two studies, one by Gibson (9) and one by Gagné (4), have reported an increase and then a decrease in generalized responses in a standard paired-associate learning task. The procedures of the two experiments were very similar, and apparently omissions and intrusions were possible in both. Therefore, the results of these experiments do not prove that generalization increased before decreasing. However, even allowing for the procedure used, the evidence they present for an initial increase in generalization is less impressive than first appears.

In the Gibson study, Vincent curves were plotted for correct and generalized

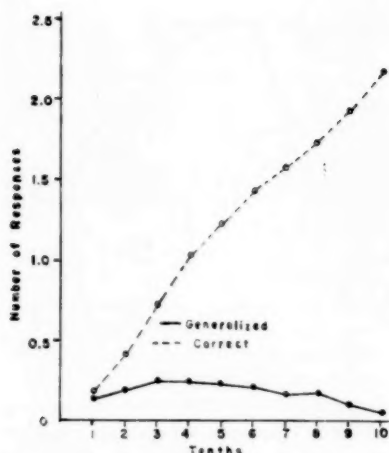


FIG. 2. Fig. 1 redrawn with curve for number of generalized responses to scale.

responses. In her Fig. 3, redrawn here as Fig. 1, the curve for generalized responses appears to do exactly what it is supposed to do, increase to a maximum early in learning and then decrease. However, this is misleading because the two curves are not drawn to the same scale. The scale of one is ten times that of the other. When drawn to the same scale (see Fig. 2) the increase in generalized responses is relatively trivial compared to the increase in the frequency of correct responses that is occurring at the same time.

The reason for emphasizing changes in generalized responses in relation to changes in correct responses is that a prior study by Hovland (11) dealt with the relative strength of a generalized response throughout learning. This was a study of generalization as a function of the number of reinforcements in classical conditioning, and was evidently influential in suggesting to Gibson the analogous hypothesis for verbal learning. Hovland measured the strength of a conditioned GSR after 8, 16, 24, and

48 paired presentations. Actually, the absolute strength of the generalized response increased with the number of reinforcements, but as it increased less at the higher levels than the strength of the conditioned response, there was eventually a relative decrease in generalization.

In the Gagné study (4) the errors also increased to a maximum and then decreased. Table 1 shows the number of correct responses, the number of generalized responses, and the ratio of correct to generalized responses for Group 1, the group that demonstrated the greatest effect. As the ratio shows, there was a slight initial increase in generalized responses. However, the increase was only from 3.11 to 3.27 while the decrease was far larger, from 3.27 to 0.22. So here, as in the Gibson study cited above, on a relative basis the increase was not impressive. And, it is felt, had omissions and intrusions been ruled out, there would not have been any increase at all in generalized responses.

TABLE 1
TRIAL-BY-TRIAL RECORD OF NUMBER OF
GENERALIZED RESPONSES, CORRECT
RESPONSES, AND RATIO OF
GENERALIZED TO CORRECT
RESPONSES*

Trial	Generalized Responses	Correct Responses	Ratio
1	2.27	0.73	3.11
2	3.87	1.20	3.22
3	4.80	1.47	3.27
4	4.93	2.33	2.12
5	4.40	3.80	1.16
6	4.73	3.53	1.34
7	4.73	4.20	1.13
8	4.13	4.87	0.85
9	3.67	5.40	0.68
10	3.80	6.00	0.63
11	2.67	6.20	0.43
12	2.47	7.20	0.34
13	1.73	7.67	0.23
14	1.80	8.20	0.22

* For Group 1 from Table 1 and Table 2 of Gagné (4, pp. 65 and 67).

In addition to the experiments of Gibson and Gagné, there is an experiment by Gagné and Foster (6) which also appears to confirm the Gibson hypothesis. This was a choice reaction-time experiment in which Ss were given 0, 10, 30, or 50 trials on two of the four components of the final task prior to 60 trials on the complete task itself. Gagné and Foster used the number of errors as a measure of generalization, and reported two main findings which appear to support Gibson. First, the total number of errors on Task 2 increased and then decreased as a function of number of trials on Task 1. That is, the 10-trial group made more errors, but the 30-trial and 50-trial group made fewer and many fewer errors than the 0-trial (control) group, respectively. This increase and decrease in the total number of errors is shown clearly in their Fig. 4 (6, p. 60). The second finding in support of Gibson is that the point of maximum errors (i.e., maximum generalization) appears to shift to the left as practice on Task 1 increases. As they report,

... the point of maximum generalization [abbreviated PMG] seems to be passed in the zero-trial group at about the 40th trial; in the 10-trial group at about the 30th trial; in the 30-trial group at about the 15th trial. The group having 50 trials of preliminary training does not exhibit any marked drop in error score (6, pp. 60-61).

Not only do generalization tendencies increase, according to Gibson, early in learning but also—and this is what Gagné and Foster seem to overlook—after the PMG they should decrease. Assuming comparable groups, assuming that training on Task 1 makes the PMG occur earlier on Task 2, and assuming that the more training on Task 1 the earlier the PMG, then according to Gibson's theory a group with more training on Task 1 should make more errors on Task 2 before its PMG than the other

group. However, after the PMG of the second group, the first group should make fewer errors. For instance, the 0-trial group reached its PMG at 40 trials and the 10-trial group reached its PMG at 30 trials. Before 30 trials the 10-trial group should make more errors than the 0-trial group. However, after 40 trials this should reverse; the 0-trial group should make more errors than the 10-trial group. (The relative frequency of errors between the two PMGs cannot be predicted without knowing the exact shape of the curves.)

If this reasoning is correct, then it is inappropriate to compare groups in terms of total number of errors made in Task 2. Instead, it should be predicted that the group with the earlier PMG should make more errors early but fewer errors late in Task-2 learning. Since Gagné and Foster report the number of errors for each group in blocks of ten trials each, and since they estimate the PMG of three groups, it is possible to use their data to test Gibson's theory in this way. Each of the three groups was compared with the other two, and the direction of the predicted difference in errors for each comparison is shown in Table 2. In each case the prediction was based solely on the relative position of the two PMGs, and no predictions were made for the trials falling between the two PMGs. The actual results were determined from Fig. 3 (6, p. 59) in Gagné and Foster's article. Each prediction was considered either correct or incorrect, and this is shown in Table 2 by plus and minus signs. As can be seen, the predictions based on Gibson's theory are correct seven times and incorrect five times. Thus, the theory would not seem to be appreciably better than chance. So here, as in the other experiments, the results fail to provide clear-cut proof that generaliza-

TABLE 2
PREDICTIONS AS TO WHICH OF TWO GROUPS SHOULD SHOW MORE
ERRORS IN EACH BLOCK OF TRIALS^a

Comparisons	Blocks of Ten Trials					
	1-10	11-20	21-30	31-40	41-50	51-60
0-trial with 10-trial	0 < 10(+)	0 < 10(+)	0 < 10(+)	—	0 > 10(-)	0 > 10(-)
0-trial with 30-trial	0 < 30(+)	—	—	—	0 > 30(-)	0 > 30(-)
10-trial with 30-trial	10 < 30(-)	—	—	10 > 30(+)	10 > 30(+)	10 > 30(+)

^a A dash indicates that no prediction can be made.

tion tendencies increase to a maximum and then decrease.³

PREDICTIONS

The question of whether or not generalization tendencies increase early in learning is of some importance in that the two points of view result in different predictions. In a transfer situation with an A—B, A—C paradigm, *S* is learning a set of responses to old stimuli. If, as Gibson says, a low level of practice on A—B results in an increase in generalization, then it should follow that negative transfer would occur; a little practice would be worse than none at all. On the other hand, if generalization only decreased there should be no negative transfer under any conditions, only positive transfer increasing as the amount of practice of the first task increased. Gibson discusses this case in her Proposition XV, "Learning of Pre-differentiated Items" (8, p. 222), where she points out that positive transfer should occur in the learning of well-differentiated items.

³ Three other experiments (2, 5, 7) in this series of studies varied the degree of first-task learning so as to permit a test of the Gibson hypothesis. In not one of these three experiments was there any indication that a low level of first-task learning resulted in an increase in errors on the second task. Actually the reverse was generally true; in all three experiments the more practice on the first task, the fewer the errors that usually occurred early in the second-task learning.

Studies in verbal learning using an A—B, A—C paradigm cannot be used to test this prediction because the interlist response interference may obscure any effects due to predifferentiation. However, the so-called "stimulus predifferentiation" studies (1, 14) would appear to provide an ideal test for these predictions. The typical predifferentiation study uses an A—B, A—C paradigm with a relatively high level of intralist stimulus similarity, thus maximizing the possibilities for reduction of generalization tendencies. Low interlist response similarity is also common, and this minimizes the possibilities of associative inhibition. Thus, in Task 1 *S* may have to learn names for complex visual stimuli, while in Task 2 *S* has to learn simple motor discriminative responses to the same stimuli (15). According to Gibson, as degree of Task 1 learning increases, negative transfer should increase to a maximum, then decrease, giving way to positive transfer with sufficient practice on Task 1.

Recently Arnoult has made a survey of predifferentiation studies, and concludes that there is "enough agreement in results among various experiments to provide generalizable conclusions in two broad areas" (1, p. 339). Fortunately, one of these two areas is the amount of predifferentiation training, and his generalization is, "*Positive transfer from stimulus predifferentiation training may*

be expected after a minimum of 4 to 8 pretraining trials and reaches a maximum after 8 to 12 pretraining trials" (1, p. 341). Further on in the same article Arnoult discusses the specific prediction resulting from Gibson's hypothesis, and states (1, p. 344) that there is no evidence ("negative results") for negative transfer. Thus, the studies reported to date give no support to Gibson's position. On the other hand, they are consistent with the viewpoint that generalization is greatest at the start of learning.⁴

SUMMARY

Gibson has suggested that during the course of paired-associate learning generalization increases to a maximum and then decreases. It is suggested here that generalization is maximum at the start of paired-associate learning and only decreases with practice. Changes in generalization cannot be inferred from changes in overt errors in the typical paired-associate procedure, for omissions and intrusions can occur, and these introduce a systematic bias. When the procedure is such that neither omissions nor intrusions can occur, the frequency of incorrect responses does provide a suitable measure of generalization, and under these conditions if learning occurs generalization can only decrease with practice.

One prediction that results from the position presented here is that with an A-B, A-C transfer paradigm and low interlist response interference, positive

transfer should increase as the degree of first-task learning increases. Studies of stimulus predifferentiation provide an appropriate test, and the results of these studies appear to support this prediction.

REFERENCES

1. ARNOULT, M. D. Stimulus predifferentiation: Some generalizations and hypotheses. *Psychol. Bull.*, 1957, **54**, 339-350.
2. BAKER, KATHERINE E., & WYLIE, RUTH C. Transfer of verbal training to a motor task. *J. exp. Psychol.*, 1950, **40**, 632-638.
3. BUGELSKI, B. R. *The psychology of learning*. New York: Holt, 1956.
4. GAGNÉ, R. M. The effects of sequence of presentation of similar items on the learning of paired-associates. *J. exp. Psychol.*, 1950, **40**, 61-73.
5. GAGNÉ, R. M., & BAKER, KATHERINE E. Stimulus predifferentiation as a factor in transfer of training. *J. exp. Psychol.*, 1950, **40**, 439-451.
6. GAGNÉ, R. M., & FOSTER, HARRIET J. Transfer of training from practice on components in a motor skill. *J. exp. Psychol.*, 1949, **39**, 47-68.
7. GAGNÉ, R. M., & FOSTER, HARRIET J. Transfer to a motor skill from practice on a pictured representation. *J. exp. Psychol.*, 1949, **39**, 342-354.
8. GIBSON, ELEANOR J. A systematic application of the concepts of generalization and differentiation to verbal learning. *Psychol. Rev.*, 1940, **47**, 196-229.
9. GIBSON, ELEANOR J. Intra-list generalization as a factor in verbal learning. *J. exp. Psychol.*, 1942, **30**, 185-200.
10. GOSS, A. E. A stimulus-response analysis of the interaction of cue-producing and instrumental responses. *Psychol. Rev.*, 1955, **62**, 20-31.
11. HOVLAND, C. I. The generalization of conditioned responses. IV. The effects of varying amounts of reinforcement upon the degree of generalization of conditioned responses. *J. exp. Psychol.*, 1937, **21**, 261-276.
12. HOVLAND, C. I. Human learning and retention. In S. S. Stevens (Ed.), *Handbook of experimental psychology*. New York: Wiley, 1951.
13. HOVLAND, C. I., & KURTZ, K. H. Experimental studies in rote-learning theory:

⁴ Goss (10) incorporates the Gibson concept of increase and decrease in generalization into his theoretical system, and makes the prediction that high similarity stimuli will actually result in less transfer than stimuli of medium similarity. Here too the prediction from Gibson's theory seems to be wrong; in a recent study (15) in which similarity was varied, the high-similarity group was the only group showing significant positive transfer.

- X. Pre-learning syllable familiarization and the length-difficulty relationship. *J. exp. Psychol.*, 1952, **44**, 31-39.
14. MURDOCK, B. B., JR. Transfer designs and formulas. *Psychol. Bull.*, 1957, **54**, 313-326.
15. MURDOCK, B. B., JR. The effects of task difficulty, stimulus similarity, and type of response on stimulus predifferentiation. *J. exp. Psychol.*, 1958, **55**, 167-172.
16. OSGOOD, C. E. *Method and theory in experimental psychology*. New York: Oxford Univer. Press, 1953.
17. UNDERWOOD, B. J. *Experimental psychology*. New York: Appleton-Century-Crofts, 1949.

(Received March 14, 1958)

TOWARD A UNITARY THEORY OF PERCEPTION

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Perceptual psychology today inclines toward phenomenological terminology. It deals chiefly with impressions of experienced reality. Recently perceptionists have been interested in physiological correlates of these impressions. Because very complicated physiological relations underlie even the primary or simple perceptual processes, is it not natural, then, that psychological terminology should be tentatively phenomenological? Only consider a few of the contents of immediate experience that can be easily described in phenomenological terms—round, complex, closed, threatening, balanced, precise, adaptive—which are functionally dependent on very complicated underlying structures. In the words of Eino Kaila, "Considered functionally the Gestalt quality is always very complicated: actually it can be simply described" (4, 5). Also from modern personality psychology one can reach the same conclusion. Personality diagnosticians employ descriptive (phenomenological) expressions from colloquial speech which often represent rich unanalyzable manifolds of functional relationships.

The problem of a unified explanation of our perceptual world implies a research program to investigate the conditions of our impressions of objects. Why do we assume a "substance" or a "material" as the carrier of perceptual thing-quality? The phenomenal object gains in reality when the impression is multidimensional. The material in question must not only offer itself optically, but also preferably tangibly. We adopt in this respect the classical reality criteria of St. Thomas. By means of multidimensional analysis we can, in my

opinion, also achieve scientific progress. However phenomenological the terminology, we can probably discern certain systems of relations by means of an analysis of our coordinated sensations, thus avoiding the diffuse impression of "substance." When proceeding to map these systems of configurations in terms of quantitative relations, the science of psychology can make progress towards becoming a natural science.

In my opinion, the most important contribution to this problem was made by David Katz in *The Modes of Appearance of Colours and Their Dependence on Individual Experience*, 1911, later revised as *The World of Colour*, 1930 (7, 8). When he described the phenomena of "surface color," "film color," and "space color," he could not guess how important these ventures would later prove to be for psychological theory. When he referred to the meaning of the reduction-screen and formulated his principle of "field-size," he implied a setting rich in the relational theory of explanation, to which we today are indebted for the possibility of intermodal theory. We must give Katz the credit he deserves when he maintains, in his *Gestalt Psychology* (9), that we previously were concerned too exclusively about the psychology of the visual sense. Probably there is for this preference a perceptual-psychological explanation. There exists in the optical realm the possibility of combining certain cues for "thingness" with more elementary color qualities—for example, microstructure of the surfaces, form and size relations, general lighting, etc. The perceived colors can, in other words, be exclusively related to optical

phenomena. The conditions of object-perception can consequently be reduced by operations not exceeding the optical sphere. And vice-versa: one can objectify "pure" color sensations without leaving the optical modality; one simply coordinates them with form impressions. The leading position of the visual sense makes this many-sidedness possible. There is no reason why color impressions should immediately be referred to other modal contents, for optical form and space are best adapted to "take them over."

Similar considerations apply to the perceived world of touch. In his *Structure of the Touch-World* (7), Katz describes the modes of appearance of tactual experience. How is this modality objectively rendered (objectivized) in terms of palpable images? Only men with normal visual images (not the born-blind) are able to objectivize their exclusively tactual impressions. Their optically constructed coordinate-system is obviously able to supply information about simultaneous shape-gestalts in the environment.

Let us consider some examples:

Surface-touch provides us a thing-impression whatever the position and the distance of the touched object.

Space-filling touch will be directly experienced in walking in a strong wind. A similar experience is also felt when one moves one's extremities quickly through a liquid. These cues hardly provide a basis for objectivizing, for a definitive object cannot be experienced. Therefore one could consider this space-filling phenomenon as an analogy of film color.

Space-filling surface quality can bring with it objectification, especially as it concerns itself with a relatively small hard object behind the material in question. The situation cannot be compared with the perception of a surface color "through" or "behind" colored illumi-

nation. The configuration brought out by illumination has its counterpart in the working of the tactual (kinematic) or haptic framework. The movement of the hands or of the felt surface yields an articulation comparable to the effect of illumination in vision.

The born-blind reach a certain state of phenomenal objectivity on the basis of their simultaneous haptic and kinesthetic impressions. But, as is well known, their tactile-kinesthetic gestalten remain only formless and diffused (12).

The sense of hearing can mediate thing-impressions only if the auditory stimulation comes combined with optic or haptic stimuli simultaneously and is localized in the same direction. The tendency to identify auditory impressions with visible things is shown in spiritualistic seances when ventriloquist mediums create illusions of sounds issuing from megaphones. In discussing objectified noises we find for the first time the attempt to deal with circumstances that Katz had not discussed in any of his works; various stimuli (noise, smell, taste, and pain) can be combined to yield "thing-characteristics," but only if *intermodal* qualities are involved. On the other hand, the dimensions of reality, e.g., of "noise-objects," can be reduced, so that finally only the pure noise remains. As is commonly known, such "pure sensation" is never found in natural surroundings. It can only be experienced in laboratories as a result of artificial refinement. The objectivation of noise depends therefore either on optic or tactual stimuli simultaneous with the auditory quality. Nevertheless, sounds do have phenomenal quality. The tone-colors of different musical instruments are evidently not alike. The strongest loudness-invariance manifests itself in those tone qualities that are functionally connected with the most complex relation-systems, as in the case of the human voice (14).

Katz's fruitful idea can also be applied to smell, taste, and other types of perceptions. I have also distinguished phenomenal modes of appearance for these modalities. Henning (3) describes several objectivizing and resolving processes in the sphere of smell sensation. I have postulated *floating smell* and *objectified smell* as phenomenal modes, and correspondingly *swelling taste* and *objectified taste impressions*. A floating smell and a swelling taste can only become objectified with parallel optic and haptic stimuli, respectively. The crux of the question is found with warmth, cold, and pain perceptions. Warmth and cold may remain isolated in floating or condensed states, or can be objectified with simultaneous optic or haptic impressions. Pricking pain (but not other forms of pain) is objectified (1), in part, in terms of stimulation provided by stabbing instruments (needle, knife, etc.) because of the added tactile and perhaps also the form component.

Our phenomenal object-configurations are, therefore, for the most part intermodally determined. It is impossible for us to imagine a nonintermodal world of things. The different modal systems do not seem so hermetically sealed from each other as was thought in the days of J. Müller. The senses constitute a single system. For man, undoubtedly, the world of sight is the most important, but surely a purely visual world would be an abstraction. Such a thing is never found in empirical reality. It is, for example, impossible on the basis of visual impressions, when the skin is anaesthetized, to decide when and at what point the surface of the body becomes excited above the threshold. However, the perception of one's bodily orientation in a dimly lit room is a function of a visual ideational structure.

But there are also cases where the haptic and vestibular controls of the optical organization are in conflict. Only

harmony between the input systems in question can restore the equilibrium of the perceptual field. Evidently these optic and haptic components possess special integrative functions for the "real" gestalts. The reality-impression of objects, their material and substantial character, is probably grounded in the repeated anchoring of phenomenal objects. These "moorings" arise from the coordination of the perceptions with basic optic and haptic elements, that is to say, from the process of objectivizing. "Rigid objects" seem to depend on co-operation of the visual and auditory senses. It seems evident that no gestalt occurs on the basis of taste, smell, vibration, warmth, cold, or pain impressions. Acoustic impressions can pass over into gestalten, but the time factor is then an organizing element. Temporal organization yields a dynamic, successive gestalt. In this, the "corticalized level" of the auditory sense plays a decisive role. Smell and taste do not have "melodies" because of the impossibility of object-gestalten at this level.

From the foregoing we can conclude that organized perception (that is to say, at the cortical stage) is really a system of relationships. Evidently we react to certain *relations* in which we—phenomenally speaking—experience contents. In this concrete relational environment there exist, to be sure, various specific stimuli. They exert their influence on our afferent nerves. That is the *physiological* side of the perceptual process. The psychological side of the process implies that the organism reacts as a whole to a relational system of stimulus-effects. These relational systems are almost unsurveyable in their complexity, e.g., with respect to the functional foundations of a perceptual object. Until recently we have been able to analyze quantitatively these complex wholes only with respect to some details of their appearance. Every stimulus is, to

paraphrase Metzger, a stimulus-system, besides having its specific function. It also merges with the whole stimulus-system in its totality (11). From this fact it follows that we get firmer and more exact perceptual achievements, the more the sporadic stimulus-components concurrently connect themselves to a framework. In this sense the well known facts of phenomenal object-constancy (size, form, and color constancy) depend on a richly organized pattern of stimulus-relationships.

We have already mentioned Katz's statement about "field-size." The object-color seems the more "retinal," i.e., more nearly pure film color, the smaller the field in which it is presented. Katz could thus establish that the color constancy of perceived objects under several colored illuminations increases as the field of view either is enlarged or seems to be enlarged. Metzger has shown convincingly that these findings *do not* depend on the limitation of the *field of vision*. The opportunity to freely look around is, of course, important, since better possibilities of separate analyses of the constant chromatic components of the illumination are then at hand. Free observation with moving eyes affords the perception of numerous "systematic stimuli." But the impoverishment of this clue can also be obtained by kinds of reduction other than by cutting down the field of vision. Metzger's beautiful experiments with the homogeneous total field show that we can give to the field of vision *any* size without obtaining thing-constancy (10). The articulation of the stimulus-field is decisive. Minimal articulation can also be a consequence of too weak an illumination. In this way "reduction" of color can be brought about.

When we say that Katz is right, we must comprehend generally the validity of what he has formulated. It is valid for more than vision only. The con-

cept of "reduction" is applicable to all senses for which objective patterning exists. The effect of the visual "reduction-screen" is really only a special case. We meet everywhere in all stimulus situations invariant perceptual objects, previously encountered and formed. And, analogously, we always find a declining degree of invariance when the reciprocity of the concomitant stimuli is reduced by any method. We must not think that this reduction can be obtained by identical technical means in all the different sense-spheres. The multiple stimuli for vision are obtained by the scope of the field of view or by the illumination of the field of view. For haptics, it is through the kinesthetic sense, for instance, the finger tips. For the invariance experiments in olfactory and gustatory perception, a reduction can only be obtained by reducing the possibilities of *objectivation* (through gradual elimination of vision and touch). The meaning of "articulation stages" can be very beautifully clarified by experiments on loudness-constancy with changing distance. The human voice is the most constant and least variable sound, while a buzzer-sound has the lowest invariance level (14).

We now leave this train of thought and occupy ourselves with some related points of view. To be perceived, objects must produce spatial location. The reaction to the "relational system" that has been indicated above predicts that we move into our perceptual products in certain invariant coordinations. The development of the subjective coordinate system belongs among the most interesting chapters of the psychology of perception. The whole field of perception possesses the same validities as we have established through the "unique thing." A "bare object" represents a peculiar if not limited relational system. The localization of the perceptual objects occurs in connection with the

three orthogonally constructed interacting planes, the vertical, horizontal, and sagittal. The medium line of the perceptual frames is assigned to these coordinations with amazing accuracy. The phenomenal local signs, "right," "left," "before," "behind," etc., apply to this system. The picture which we experience to our right remains "the same" when we turn our eyes and locate it to "the left." By so doing, we have joined the time dimension to the coordinate system. The example of the picture demonstrates the invariance of the object in time.

Once more: Because the precise and clearly experienced external world supposes a mediating relational system in organized detail, its concreteness and stability are completely lost if we experimentally reduce the optical-acoustical-haptical stimulus pattern to a minimum. In experiments with movement perception (13) such loss can be demonstrated. If the subject, for instance, is told to estimate the speed of an evenly illuminated paper square which moves sideways with uniform speed in the frontal plane, one thereby finds considerable variance between the successive estimations when the experiment is carried out in a completely dark and "reference-poor" environment for the motion. If more richly organized reference-criteria are admitted (normal daylight, "passing things," structured background, and the like), one immediately judges with greater accuracy and with decrease in the variance of the observations. The largest overestimations of the speed are elicited by the most reduced configurations. One lone clue, a stable light source in a dark room, is often enough to bring about a considerable decline in the errors (about 15%) and the variance (about 5%).

Through another experiment at the Psychological Institute of the University of Helsinki, it was found that the

subject succeeded essentially in a "localization task" with sound sources in a dark room if he could use a "reference sound source" which was attached to a slightly movable long pole between the subject and the "goal." The subject himself could "regulate" this sound-source (buzzer). Most effective was the "acoustical-optical method," whereby the pointer was also illuminated (14).

Of all the phenomena which produce a permanence in the frame of the space-time-coordinate system, our empirical self is probably the most important. We notice most distinctly the temporary interference and functional crises of the perceptive-psychological system in the not-too-rare cases where we wake up in unknown places, e.g., in darkness. All conceivable clues for the perceptual function seem to be absent—even the possible pressure experiences and kinesthetic impressions which are recalled by our position in bed. And nevertheless self-permanence, which we doubt least, shapes the perceptual content—apart from some pathological conditions. According to Gadelius (2), the empirical self will, through this temporal invariance, be constituted as a perception phenomenon with the perceptual components of the body-image. It is one of the highest acquisitions of perception. Psychology has doubtless shown that the invariance of the self-experience depends on conditions similar to the conditions responsible for the perception of *objects*. Kaila observes in this connection:

The so-called "phantom limbs" of the amputated part of the body show what tenacious constancy and hallucinatory intensity this body-scheme has: a war invalid for instance, the one who has lost his leg, is initially exposed to danger because he experiences hallucinations of his lost leg, so that when he gets up out of bed, for example, he makes a step with the non-existing leg (5, p. 164).

With the most trivial and commonplace perceptual systems—the phenomenal objects—as a starting point, we have proceeded to the most complex thing that a human being is able to perceive at all: his empirical self. The main conclusion to be drawn from our discussion is obvious: It is the stimulus pattern as a system of relations, not as representing some “substantial stuff,” which immediately gives rise to the impression of a three-dimensional world.

In summary, I believe the position represented here paves the way for modern perceptual psychology to arrive at functional principles of explanation.

REFERENCES

1. BUYTENDIJK, F. J. *Über den Schmerz*. Bern: Hans Huber, 1948.
2. GADELIUS, B. *Human mentality in the light of psychiatric experience. An outline of general psychiatry*. Copenhagen: Levin & Munksgaard, 1933.
3. HENNING, H. *Der Geruch*. Leipzig: Barth, 1924.
4. KAILA, E. *Logik und Psychophysik*. Ein Beitrag zur theoretischen Psychologie. *Theoria*, 1944, 10, 91–119.
5. KAILA, E. Emotion, vilja, personlighet. In D. Katz (Ed.), *Handbok i psykologi*. Stockholm: Bonniers, 1950.
6. KATZ, D. Der Aufbau der Tastwelt. *Z. Psychol.*, 1925, Ergbd. 11, 1–270.
7. KATZ, D. Der Aufbau der Farbwelt. *Z. Psychol.*, 1930, Ergbd. 7, 1–484.
8. KATZ, D. *The world of colour*. London: Kegan Paul, Trench, Trubner, 1935.
9. KATZ, D. *Gestalt psychology*. New York: Ronald Press, 1950.
10. METZGER, W. Zur Phänomenologie des homogenen Ganzfelds. *Psychol. Forsch.*, 1929, 13, 6–29.
11. METZGER, W. *Psychologie. Die Entwicklung ihrer Grundannahmen seit der Einführung des Experiments*. Heidelberg: D. Steinkopf, 1954.
12. RÉVÉSZ, G. *Die Formenwelt des Tastsinnes. Grundlegung einer Haptik, und der Blindenpsychologie I-II*. Haag: Martinus Nijhoff, 1938.
13. TAKALA, M. Asymmetries of the visual space. *Ann. Acad. Sci. Fenn.*, B72.2, 1951.
14. VON FIEANDT, K. *Varseblivningens psykologi*. Stockholm: Natur & Kultur, 1956.

(Received April 10, 1958)



CONVIVIUM
ESTUDIOS FILOSOFICOS
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CONVIVIUM appears twice a year, and contains articles, notes and discussions, book criticism, and an index of periodicals. Articles are printed in the Latin languages, with a summary in English and German.

Editor: JAMIE BOFILL BOFILL (Professor of Metaphysics)

Price: Single copy, 60 pesetas (Foreign, \$2.40)
Subscription, 100 pesetas (Foreign, \$4.00)

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